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RATIONALIZATION OF THE LITERATURE OF SCIENTIFIC RESEARCH

IN the forty years since Prof. W. A. Noyes, representing the American Chemical Society, outlined to representatives of the Chemical Society of London and the Society of Chemical Industry a plan of co-operation in the establishment of a really comprehensive abstracts periodical for chemical science, the need for effective means of securing the dissemination of scientific knowledge has become much more urgent. Difficulties felt first in chemistry are equally acute in other sciences. The last edition of the "World List of Scientific Periodicals" recorded some 33,000 different titles, and although a number of periodicals have been suspended during the War and possibly may never resume publication, many new scientific and technical periodicals have been issued which do not appear in that list.

The limitations of the present abstracting and indexing arrangements have been repeatedly discussed during the past ten years or more without as yet any more tangible result than Prof. Noyes' approach to the British societies in 1905. An informal conference in July 1931 initiated by Sir Henry Tizard, then president of the Association of Special Libraries and Information Bureaux, and a further inquiry carried out by that Association led to no real improvement; and the significance of Dr. S. C. Bradford's comments, as he originally gave them in *Engineering* in January 1934, or at a subsequent conference of the Association of Special Libraries and Information Bureaux in September 1937, on the existing overlap among the three hundred abstracting and indexing journals, and his demonstration that only about one third of the important scientific papers are noticed by the abstracting journals, have largely been overlooked by scientific workers. Nevertheless, the law of scattering deduced by the late Mr. E. Lancaster-Jones and described in papers at that conference is an outstanding factor which should compel a radical change in methods of abstracting and indexing, if not in the whole system of publication.

Prof. J. D. Bernal discussed the matter at some length in 1939 in his book "The Social Function of Science", but although his criticism was even more fundamental and insisted on the need for a drastic revision of the whole system of scientific communication, the suggestions he advanced, like the project of the American Documentation Institute for Scientific Publication and Bibliography which is given in an appendix to Prof. Bernal's book, appear to have received little attention or discussion in Great Britain. Meanwhile, the subject has engaged the attention of the Royal Society, as was shown in the report of the British Commonwealth Science Committee, and Dr. Joseph Needham's memorandum on an international science co-operation service suggests that it will not be overlooked in the activities of the United Nations Educational, Scientific and Cultural Organisation. Mr. Freemont Rider's book "The Scholar and the Research Library" (see *Nature*, **154**, 655; 1944) and

Mrs. L. Moholy's article on "Documentary Reproduction" (*Nature*, **156**, 24; 1945) are further pointers showing that fundamental issues are being faced, and the possibilities of developing new techniques to overcome the difficulties are being examined.

Prof. Bernal is not alone in challenging the basis of the present system of publishing scientific papers. Zeliaette Troy, librarian of the Boyce Thompson Institute for Plant Research, discussed the whole question in an article "Streamlining Production and Distribution of Current Periodical Articles" in *Special Libraries* of July–August 1943, while in the first issue of the *Journal of Documentation*, Mr. N. W. Pirie, of the Rothamsted Experimental Station, outlined two tentative proposals, less radical than those of Prof. Bernal but offering some prospect of advance, for the publication of scientific papers. Thus, when Prof. Bernal returned to the subject in his address at the annual conference in September 1945 of the Association of Special Libraries and Information Bureaux (*Nature*, **156**, 605; 1945) the ground was much better tilled for his ideas.

Prof. Bernal has rightly stressed the critical importance, for the man of science, of being able to read, rapidly and in a convenient form, at every stage in his work, the results up to date of all relevant scientific investigation. The production of scientific publications under the present system has long outstripped the capacity of scientific workers to read more than a small fraction of the papers in a field itself a very small part of science. The defects of the existing abstracting systems are recognized; but from what has already occurred, it seems very unlikely that any radical re-organisation in these will cover the large gap caused by the law of scattering which the late Mr. Lancaster-Jones and Mr. H. T. Pledge formulated as a result of the investigation which Dr. Bradford instigated.

It has clearly become impossible for the average scientific worker to keep up with progress in his own field unless he devotes a major part of his time to reading; and there is grave danger of good scientific work being lost permanently because it was not appreciated when published. Many scientific men will agree with Prof. Bernal that the number of scientific periodicals is excessive, and however legitimate it may be to expect some Government support in this field, that support could rightly be made conditional on an effective measure of rationalization which would eliminate some of the redundant periodicals and make more effective use of those remaining, as well as avoid some of the present waste of time and money and the source of irritation to scientific men themselves. Without such Government support the probability of a complete breakdown of the present system on account of expense is real.

The first step, as Prof. Bernal indicates, should be some fundamental thinking as to the purposes to be served and the needs of the various categories of scientific worker by whom scientific publications will be used. Generally speaking, it may be said that full information regarding a particular piece of work will only be required by a limited number of workers; for others the information will be needed in outline only, if at all. A system of limited distribution of full papers and a wider distribution of summaries or abstracts are the essential features of a system adapted to working needs; but this would require supplementing by a system of regular reports or monographs covering the sum of recent advances in any field, as well as by a body of readily accessible archives in which reference could be made to the work of the past. The system must also provide for the production of the full paper, not merely at the time of issue but also at a later date for an indefinite period.

This view reduces the problem essentially to one of the distribution of the appropriate units, and for this purpose the individual scientific paper and an appropriate abstract of the paper are the fundamental units. At the same time, it links up the question of publication of scientific papers with the question of abstracts and the publication of reviews, monographs and reports as different aspects of one problem of securing the most rapid and effective dissemination of the results of scientific research with the financial resources available. Here it is essential, as Zeliaette Troy has pointed out, to distinguish between the working tools of the individual man of science and his browsing literature. In his paper last autumn before the Association of Special Libraries and Information Bureaux, Prof. Bernal indicated that an adequate scheme would involve detailed consultation between men of science, librarians, editors and publishers, and he looked to the librarian to do much more than in the past to ensure that material was sent to him in appropriate form for handing out to the scientific investigator.

Prof. Bernal suggests first that the primary unit in scientific publication is the individual scientific paper dealing comprehensively with one topic. Books and other larger units may be considered as an accumulation of papers, while notes and 'letters' are papers in embryo. The organisation of publication and recording should, in his view, centre on that of the scientific paper: the journal has ceased to be a really satisfactory means of distributing scientific information. While, as already indicated, it fails to provide the man of science with all the papers he requires, it presents him with a large number which are of no, or little, interest to him; possibly a journal containing one paper which bears on his work may contain twenty or thirty others which do not concern him at all.

Under the scheme forwarded by Prof. Bernal, each country would have a centre of scientific publication and exchange, which would receive from societies papers already passed by referees as suitable for publication and lists of members to whom papers from any part of the world on specified subjects or groups of subjects should be sent. The papers would themselves be sent in originally by research workers to the publication office and submitted to an editorial board of referees as at present, and if passed would be reproduced suitably—photographically was originally suggested by Prof. Bernal. The national centres would act as clearing houses for these papers, arranging for their internal distribution and sending others in blocks in appropriate numbers to the clearing

houses in other countries for distribution to the scientific men there. Payment of a subscription to one society would entitle the subscriber to the services of the whole organisation.

Such a system might well be economically self-supporting and could make a much better claim, if need be, for government support than any of the present organisations. Moreover, as Prof. Bernal notes, scientific workers would be relieved of a good deal of the purely technical editorial work which at present falls on their shoulders. A small central staff could deal efficiently with this, and for the same expenditure far more papers could be published, if desired, than at present. Furthermore, papers of exceptional length or possessing other expensive features could be published on this system without extra difficulty.

A similar service but with a much wider network could be organised for abstracts, and here Prof. Bernal leans to preparation of the abstract by the author, verification by his own scientific society and emendation if need be for the sake of uniformity. Further abstracts might, however, also be required from papers of value to several fields of science for distribution in such fields. All such abstracts should be produced in form suitable for modern filing systems, and Prof. Bernal would go straight to a card system, arranging for complete sets of abstract cards to go to libraries and institutions and for selections to be sent to research workers. Similarly, he suggests that the scientific societies should make themselves responsible for the production of monographs and text-books, though leaving wide freedom for original work of an unorthodox kind. Prof. Bernal recognizes that any such scheme will require a great deal of detailed elaboration and study; but without something of the kind he holds that the literature of scientific research will be choked by its own productivity. Moreover, as Mr. Pirie's article indicated, it is possible to utilize a good deal of the existing organisation of scientific societies and publishing bodies. A start on an international basis is, however, particularly important if the full value of the system is to be secured, and on this count alone it is to be hoped that the situation will receive early and impartial consideration either by the International Scientific Unions or the United Nations Educational, Scientific and Cultural Organisation.

Objections to any such schemes as Prof. Bernal and Mr. Pirie have outlined can at once be foreseen—the proposals are not in essence new, it should be remembered. Ideas of the same kind were advanced by Prof. W. Ostwald in a Memorial on the Foundation of an International Chemical Institute published in *Science* within a few days of the outbreak of the First World War and afterwards in further detail in chapter 8 of his book "Die Chemische Literatur und der Organisation der Wissenschaft" (Leipzig, 1919). But such schemes at least have the merit of offering a reasonable chance of securing complete cover of the scientific literature, and eliminating the scattering and burying of important papers, and at the same time lightening the burden of the task of keeping abreast of the literature which at present falls upon the individual man of science; for they would reduce the number of scientific periodicals to which he is obliged to subscribe, they would correct the multiplicity and inefficiency of the present abstracting services and the costliness and inefficiency of the existing reprints practice.

As a sequel to Prof. Bernal's address, an informal committee was set up, consisting of members of the Association of Special Libraries and Information Bureaux and of the Association of Scientific Workers, to study this question. This committee has formulated three principles, which it believes should form the basis of any contemplated revision of the mechanism of scientific publication: (1) The fundamental unit of scientific communication is the individual paper containing the results of original researches; (2) the autonomy of scientific societies and other publishing bodies must remain unchallenged; (3) the financial position of publishing bodies must not be impaired by any proposed development. The committee recognizes that scientific journals have other functions to fulfil, though it may be questioned whether some of those enumerated by the committee may not be better discharged by a commercial journal than in the proceedings of a learned society. Compared with Prof. Bernal's views the committee's vision, however, seems to be unduly circumscribed. Its members have not clearly recognized the importance of the review or monograph, or the opportunities in that direction which the scientific societies are as yet using only imperfectly. While the first principle should command general acceptance, the second and third seem more likely to stifle than to encourage the unprejudiced examination of the whole question which is so imperative. The real objective is to find the best possible means of improving the system of scientific communications and to develop the most appropriate organisation for that purpose, having regard to available and potential resources. To announce in advance that, whatever the findings, the existing structure must be maintained intact is to make organisation and institutions the masters of the sciences they exist to serve, and to frustrate any hope of advance.

Discussion of the whole question alike by individual men of science, librarians, editors and publishers, as Prof. Bernal suggests, as well as by the learned societies, professional associations and various Government departments and industrial organisations concerned, is indeed desirable, and should precede the convening of a conference representing both those publishing organisations chiefly concerned and the research workers on whose behalf the reforms are most urgently needed. It is, indeed, important that the reform of scientific publications should be seriously considered while the post-war policies of scientific societies and other publishing bodies are under review, and the fact that a session at the forthcoming Royal Society's Empire Scientific Conference is being devoted to this topic will afford an opportunity for at least preliminary public discussion. But there must be general agreement not to prejudice the issue. The whole question must be explored with open minds and with the desire first and foremost to

find the most appropriate means which new developments and techniques can give us of serving the needs of science in this matter of publication. Thus approached, there need be no fear that however the forms in which publishing bodies or scientific societies serve the needs of science may change, there will not be fresh functions to fulfil in place of any that have become obsolete. The question must be explored in the same way as any other problem, and the first consideration must be to find the true answer no matter what changes in organisation and practice it may import. The whole matter is one which might very appropriately be taken up by the Association of Special Libraries and Information Bureaux for early discussion between the many interests involved.

MATHEMATICS OF THE GAME OF BRIDGE

Théorie mathématique du bridge à la portée de tous

Par Prof. Émile Borel et André Chéron. (Monographies des probabilités, Fascicule 5.) Pp. xx + 392. (Paris: Libr. Gauthier-Villars, 1940.) 175 francs.

THE second section of the fourth volume of Borel's treatise on probability bears the title "Applications aux jeux de hasard". The present work is due to the interest which this excited among bridge players and consists of a thorough investigation of the numerical probabilities which bridge presents. Such a study is of value not only to bridge players; it contributes equally to the progress of the science of probability which already owes much to the study of games of pure chance, and of which many applications are related to the theory of games where chance and the psychology of the players both enter.

The probabilities of bridge, in so far as psychology is not involved, are exact and therefore rational numbers. In this work they are given as percentages, some of them to six decimal places. Thus the statement that before the cards are dealt the probability that my hand will contain exactly one heart is 8·006 per cent, means that in every 100 hands dealt to me, I can expect to hold on an average 8·006 hands which contain exactly one heart, or in 100,000 hands 8,006 which contain exactly one heart. To get an idea of what this means in respect of time, allowing 100 deals per day at intervals of five minutes, that is, 8 hours 20 minutes a day, I can expect to hold one heart in my hand about eight times per day. The probability that in the two hands held by my partner and me there will be exactly one heart is 0·040 per cent, so that only four such combinations could be expected to occur in 100 days play at the above rate.

The first chapter devotes 28 pages to the theory of shuffling the cards, probably the first proper investigation of this matter. The second chapter is concerned with the *a priori* probabilities of particular distributions, that is to say, probabilities after the cards are shuffled and dealt but before any player has looked at his hand. Imagining the four players N, E, S, W to be seated at the table and S to be the dealer, the number of distinct deals which he can make is a number of 29 digits, sufficiently approximated by 5×10^{28}. In this enumeration the distribution which would arise, if two players exchanged hands, is regarded as distinct from the distribution before the exchange. If all the adult inhabitants of the earth formed bridge tables, say 300 million tables, and each table made 100 deals a day, it would take 50,000 milliards of centuries (5×10^{15} years) to deal the above number. There is, therefore, no hope that a particular player will ever take part twice in the same distribution.

Having regard only to the number of cards held in a suit, there are thirty-nine distinct types of hand of which the type 4–4–3–2 is most frequent with a probability of 21·55 per cent. The type of hand which contains exactly five cards of one suit and not more than five of any other suit has a probability of 44·34 per cent. If we replace five by six the probability drops to 16·55 per cent. A given player may expect to hold a hand of the types 9–4–0–0, 10–1–1–1, 10–3–0–0, once or several times in about thirty years (100 deals per day). The probability that he holds no card higher than a 9 is 0·0547 per cent, that is, about five such hands in three months.

Chapter 3 considers the modifications of the probabilities when the cards have been dealt, and each player has looked at his hand, but no bid has yet been made. The probabilities here calculated will, therefore, be modified by knowing the bids, even if the bid is a pass. They are, however, rigorously exact for the dealer who has the first bid, and this first bid often has a decisive importance. S deals and finds that his hand contains five hearts, say. Table 24 gives the probability that one of his adversaries has a void, singleton, or doubleton as 79 per cent. Table 25 gives for various distributions in S's hand the probability of an 'accident' (void or singleton) in one, two or three of the other hands. From this table it appears (1) that a long suit and not an accident provokes an accident elsewhere, (2) an accident is the rule and not the exception in a pair of hands, and still more so in the three hands, (3) an accident is the exception and not the rule in a single hand. Table 26, with the gladiatorial caption "l'espoir dans le mort", gives the chance of finding N with a specified number of the cards missing from S's hand in which he is interested; for example, S has five high cards (A, K, Q); the chance of finding at least one more in N's hand is 95·7 per cent. Table 28 gives the probability of exhausting a suit in a specified number of rounds. Thus with $A\,K\,Q$, $A\,K\,Q$, $A\,K\,Q$, $x\,x\,x\,x$ a declarer has a 77 per cent chance of making a contract of three 'no trumps'.

One of the most interesting parts of this chapter is the tables giving the defensive probabilities of certain combinations before 'dummy' is exposed. Thus the trick-making expectation of $K\,Q\,x$ is 0·978 if led, and 1·167 if not led. A defender who holds spades $A\,K\,Q$, hearts $A\,K\,Q\,J$, diamonds 4 3 2, clubs 4 3 2 can expect to make 4·4 tricks if a club is led against a diamond declaration. The lead of A from $A\,7\,x$ will lose his side 5·6 tricks in 100 cases, while the lead of 7 or x will lose 10·8 tricks.

In chapter 4 the first card has been led and 'dummy' is exposed. At this stage the hypothesis is made that each player sees 26 cards, in other words, that the first card is led face downwards; otherwise two of the players see 27 cards, so that if S is the declarer, S and E are in a slightly advantageous position. The tables are based on the hypothesis stated. One important fact is emphasized, that the probable distribution of concealed cards between the two hands which hold them is not affected by the distribution of the visible cards. Thus whether S holds 7–6–0–0 and N 0–0–7–6,

or S holds 4–3–3–3 and N holds 3–3–4–3, can afford no help to S in determining the distribution of the remaining cards between E and W. There is a popular belief in the contrary.

The tables of this chapter justify the well-known rules about the probable distribution of the concealed cards of a given suit, and the important 'law of attraction', namely, that the hand with more than half the concealed diamonds, say, has probably less than half the concealed spades, say.

Chapter 5 is concerned with the line of play which has most chance of improving the score. The numerous examples are based on the rules of scoring (i) in plafond, (ii) in contract bridge. In either case there are often two lines of possible play: (*a*) security, (*b*) risk. Setting aside all considerations of psychology of the players (often very important) the line to be adopted in any given case is that which offers the greater mathematical expectation. This calculation is complicated by the fact that the total points scored by a successful contract is scarcely ever that written on the scoring sheet. Account has to be taken of the invisible points; for example, the first game at contract is worth 450 points, 100 marked for game and 350 towards the rubber. The chances of bidding slams and the dangers of doubling such contracts are examined; the policy of part scores and game bids and of doubling and redoubling are examined numerically.

In a brief review it is impossible to do justice to the rich contents of this book, with its 134 tables, 4,000 probabilities and numerous numerical examples, all of high interest.

Whether or not bridge is the card game of kings, it is certainly the king of card games, and the English reader should feel nothing but satisfaction in conjecturing the etymology of "le grand chelem" and "le squouize".

L. M. MILNE-THOMSON

THE PROBLEM OF HUMAN AGGRESSIVENESS

Essays on Human Evolution

By Sir Arthur Keith. Pp. x + 224. (London: Watts and Co. Ltd., 1946.) 15*s*. net.

IN these forty short essays, written during 1942–44, Sir Arthur Keith develops a thesis that would be vitally important if true, and in any event raises issues it would be folly to ignore. He claims (1) that men have always lived and always will live with a mixed allegiance to two incompatible codes of conduct; one of friendliness, gentleness and co-operation within the community, the other of competition, hatred and warfare without (for example, pp. 5ff., 100ff.). (2) A community becomes a unit by isolation from its neighbours, whereby it preserves its genetic character, and it evolves as a unit (pp. 105, 130, 142). (3) The primitive unit is the tribe; the advanced one the nation–state. Increase in size is brought about by warfare, which is part of the evolutionary process (pp. 48, 173). (4) Behaviour according to the code of 'amity' is morally approved, but 'enmity' between tribes or states is the necessary condition for human evolution; therefore a universal community or state, even if possible, is undesirable, since it would stop the evolutionary process (pp. 45, 53).

In proposition (1), though he exaggerates the amity within the community and the enmity without, Sir Arthur states one of those unpleasant facts that well-meaning people like to forget and that needs to be rubbed in constantly. Aggressiveness is part of human nature; civilization has not abolished it; it has even made it more dangerous. However, Sir Arthur's mode of statement is misleading. In every phase of human conduct there is liable to be tension between opposed ends, each one good of its kind but such that both cannot be pursued together. The tension he emphasizes, though very important, is one among several. To call it, as he does, one between 'ethics' on one hand and 'evolution' or the 'cosmic' process on the other hand adds to the confusion.

Many writers misuse the term 'evolution'; but that is no reason why Sir Arthur should follow a bad example. Biological evolution in the proper Darwinian sense is the process by which the normal kind of species, of fairly definite and fairly homogeneous genetic character, gives rise to one or more daughter species of different but equally definite and homogeneous genetic character. The process is not likely to be complete in less than one hundred thousand generations (cf. J. Huxley, "Evolution", 1942, p. 61n.). Natural selection acting on a well-adapted species in a constant environment may be expected to stabilize rather than transform. Thus in the nearly constant environment of the sea many ancient species survive and appear to be very stable. On the other hand, natural selection will have a transforming effect in what is presumably an intermediate stage between well-defined parent and daughter species, where there is a varied population containing many heterozygous individuals, perhaps a high mutation-rate and perhaps a process of reorganising the gene system distinct from mutation of single genes. This transforming process, as part of the more complete process, may also be called evolution. If the environment is changing, it will go all the faster; but even so, the elimination, in a whole population, of characters producing lower viability is likely to be a matter of hundreds, if not thousands, of generations (cf. Huxley, *loc. cit.*, pp. 56–58). The term evolution in the first sense of full transformation of species cannot be applied to anything that is known to have happened to the human race since the earliest permanent agricultural settlements, 200–300 generations ago. It is true that the human race, unlike ancient species, is varied in genetic make up, markedly heterozygous and may be changing considerably. If Sir Arthur had produced any evidence on this point he might have been justified in speaking of human evolution. As it is, to call changes in human habits, tradition, culture, in what men learn in the course of their lives, evolutionary, is to make clear thinking impossible. Sometimes he speaks of human evolution where the context seems to require simply the term 'competition' (pp. 202, 208, 210, 214).

Proposition (2) above appears to be the result of confusing cultural with genetic change. Moreover, it looks as though Sir Arthur exaggerated the state of isolation and the homogeneity of primitive peoples in the interests of his theory. As to (3), if tribe or nation A conquer tribe or nation B, they do not preserve their genetic purity unless they exterminate B. This has seldom happened in the most advanced regions, Europe, Asia and North Africa, where there has been no complete genetic purity or isolation of

tribes or nations for more than 6,000 years ; not even among Jews or gypsies. Instead, conquerors and other migrants have made the whole region a genetic melting-pot. The most isolated peoples are not the most advanced. In dealing with (3) Sir Arthur confuses the issue by lumping together two different forms of amalgamation that arise out of warfare (for example, pp. 78–79) ; namely, where the *A*'s conquer the *B*'s and absorb or are absorbed by them, and where the *A*'s and *B*'s join on equal terms to fight the *C*'s. Genetically the result is not so different, but culturally very different. The second is generally considered more beneficial, at least to the *A*'s and *B*'s. This does not fit Sir Arthur's argument. It may be true for Europe that the most war-like peoples have been the most advanced; but it is not true for Asia, as Sir Arthur partly admits (pp. 177ff.).

Finally, the argument for (4), the undesirability or even impossibility of a world state or community, falls to the ground. Of course, a genuine difficulty still remains. Feelings of common interest and political loyalty have usually been the result of fighting a common enemy. If there is no common enemy, how are they to develop ? The question was well expressed many years ago by William James ; How are we to find a moral equivalent for war ? Now that warfare has become so destructive as to be suicidal the question is more urgent than ever. While criticizing the way he has done it, one must be grateful to Sir Arthur Keith for his determination to focus attention on one of the greatest of moral and political problems.

A. D. RITCHIE

INSURANCE AGAINST SICKNESS

National Health Insurance

A Critical Study. By Hermann Levy. (National Institute of Economic and Social Research : Economic and Social Studies, No. 4.) Pp. x + 366. (Cambridge : At the University Press, 1944.) 18*s*. net.

THIS work by Prof. Hermann Levy forms No. 4 in the series published from the National Institute of Economic and Social Research. The work was completed in 1942, and should have been published in 1943. Despite the fact that much has happened in the interim, the work must be regarded as a serious and very helpful contribution to the most important medico-social problem of the day.

Whatever may be the fate of the Health Bill which is before Parliament at the moment of writing, Prof. Levy's book will remain not only as a very well documented exposition of the present position, but also as a repository of carefully thought-out suggestions, the spirit, if not the letter, of which may prove useful in the future. The first part of the book is devoted to an excellent summary of the historical aspect of national health insurance from its beginnings in 1911. The author points out that Chadwick was the first to enunciate that disease creates poverty. He is critical of the work of the Royal Commission of 1924–26, mainly on the grounds that it did not carry through any serious review of the administrative system on which national health insurance is based. So far as the position at the present day is concerned, Prof. Levy puts forward the old criticisms, and some new ones, in a balanced and forceful manner. He points out that no country has yet ventured to guarantee to the sick worker full compensation for loss of earnings, and that national health insurance does not relieve the working-class family from further money contributions for sickness contingencies. The membership of approved societies covered for hospital treatment is very small. He considers that hospital treatment has probably been the greatest failure of the national health insurance scheme in Britain, and that this has not been compensated for by a growth of hospital schemes outside its ambit. In fact, Britain has more and more dissociated hospital benefit and treatment from the insurance scheme. It is well known, of course, that there are important medical services which are not covered by the scheme, and that only the barest cost of medicine is provided. Prof. Levy is inclined to labour the fact that the unsatisfactory payment of medical men has greatly restricted the interest which the ordinary panel doctor can be expected to take in any individual patient—a position which the profession has frequently denied. His criticism that existing conditions of cash benefits and treatment have caused insured persons to resort to self-medication is probably quite correct.

What are the remedies for these failings ? Despite his admission that approved societies have nowhere shown a desire to originate and pursue a constructive and dynamic policy of health improvement, and that the insurance committees have done nothing in this direction either, Prof. Levy believes that it was not national health insurance which was wrong, but the way in which it was administered. He is convinced that national health insurance should remain a separate statutory social service under an entirely different system of organisation and administration. In effect, he says, abolish the approved societies and insurance committees, and replace them by a system of territorial or occupational institutions. He therefore proposes the setting up of new administrative bodies, in the main municipal or rural, while leaving it open to large industrial and other establishments to have their own sickness funds on the same basis as the statutory funds. There would be flexibility in the rate of contribution, and the local sickness funds should set up district insurance bureaux to deal with claims, the payment of benefits, inquiries and advice. He does not believe that the shortcomings of panel practitioners are any reason for changing the system to one of State medical men. Prof. Levy therefore runs counter to Sir William Beveridge and the Bill which has been modelled on his report. Beveridge suggested a union of the statutory services which all over the world have so far been separately administered with different aims. Levy recommends the retention of the separate services. Beveridge implies that the medical service is to be divorced from cash benefits ; Levy recommends a system of integrated local and occupational funds.

It should not be thought that perusal of this book at the present time is simply equivalent to the reading of a commentary on some past and forgotten chapter in the history of social legislation. In Cicero's day *homines ad deos nulla re proprius accedunt quam salutem hominibus dando*, and the efforts made by men to give health to their fellow-men have continued —very slowly until recently, it is true—through the centuries. Should it be found necessary to reconsider within the next generation the constitution of the imminent 'medical Utopia', this volume will undoubtedly provide a sound basis for the required research.

E. ASHWORTH UNDERWOOD

Atomic Artillery and the Atomic Bomb

By John Kellock Robertson. Pp. xii + 173 + 8 plates. (New York: D. Van Nostrand Co. Inc.; London: Macmillan and Co. Ltd., 1945.) 12*s*. 6*d*. net.

TO the serious student of science this is a disappointing book. It is essentially a reprint of the author's "Atomic Artillery" with two added chapters on nuclear fission and the atomic bomb. The additional material is very slight and contains at least one unfortunate misprint, and several misstatements of fact.

In 1937, when the first edition appeared, this book probably served a useful purpose in directing the attention of the semi-technical reader to nuclear physics, at that time still an academic discipline of little practical value. So much excellent material has now been published about the basis of atomic energy, particularly in the United States, and so much has been done to cater for public interest in nuclear physics, that the present work reads as a pleasantly told but rather old-fashioned story. Like the official Smyth Report, the account given of the fission process fails to acknowledge the essential contribution to the subject made by Joliot and his colleagues in France. Their discovery of the neutrons emitted in the fission process was the crucial observation which made the chain reaction practicable. Similarly, the descriptions given of equipment used in nuclear physics, such as the Van de Graaff generator, or cyclotron, are scarcely in line with modern practice.

Notwithstanding these criticisms this book will be read with profit by many who wish to understand something of the romantic story of the growth of nuclear physics and its exploitation. The style is simple and stimulating, and the errors and omissions which occur will be noticed by few who are not nuclear physicists.

M. L. OLIPHANT

Radio Receiver Design

By Dr. K. R. Sturley. Part 2: Audio Frequency Amplifiers, Television and Frequency Modulated Receiver Design. Pp. xv + 480. (London: Chapman and Hall, Ltd., 1945.) 28*s*. net.

THIS book forms the second part of a comprehensive treatise on the principles of design of radio receivers, the first part of which was reviewed in *Nature* of June 12, 1943, p. 657. Part 2 preserves continuity with Part 1 to the extent that the chapters are numbered 9 to 16, and it completes the process of following the progress of the signal from the aerial through the receiver to the onput or loudspeaker.

In accordance with this scheme, the first three chapters of the present volume deal with audio frequency amplification, the power output stage and power supplies. Next, the necessary controls on a receiver are analysed in terms of their function of controlling automatically the gain and tuning of the receiver; while Chapter 14 reviews in some detail the definitions and specifications appropriate to the measurement of overall performance of receivers. The last two chapters in the book are each practically self-contained and are devoted to the special features associated with the reception of frequency modulated and television transmissions respectively.

The author's clear analytical style, with appropriate graphical illustration and numerical examples, has been maintained throughout this volume, which with its companion, Part 1, forms a most useful work of reference for the radio engineer and designer. In addition to the actual material in the book itself, each chapter ends with a most helpful selected bibliography, the total number of references in the present volume being nearly two hundred. The reader is thus provided with a direct key to the most useful literature, which is scattered over a wide variety of publications.

Poultry Keeping for All

By Harold Perkins. Pp. 66 + 8 plates. (Worcester: Littlebury and Co. Ltd., n.d.) 7*s*. 6*d*. net.

THIS book can be looked upon as only a brief introduction to the subject of poultry-keeping for beginners. The author has obviously had wide practical experience, and deals in a non-technical manner with matters, mostly based on personal experiences, which sometimes are not in line with scientific facts. His contact with domestic poultry-keepers has enabled him to appreciate their difficulties, and these are dealt with in a helpful way, punctuated by entertaining anecdotes.

The author admits that he does not attempt to go deeply into any particular branch and has mainly generalized, advising the reader to seek further information in more specialized works. In this connexion, it would have been helpful if a bibliography of such works had been appended.

Better use might have been made of the paper space available; on p. 23 barely one third of the space is occupied by a short paragraph headed "Breeding", which the author might well have expanded, although he states: "Of breeding I don't want to say much because there is so much to be said"! The subject of feeding is also very sketchily and inadequately covered.

The book is badly arranged, for under the heading of "Diseases" are included such subjects as "egg grading and washing", "the future of D.P.K. Clubs", and other irrelevant items. The illustrations, however, are attractive and the type good.

D. J. G. BLACK

Active Nitrogen—A New Theory

By Prof. S. K. Mitra. (Joykissen Mookerjee Medal Lecture for 1945.) Pp. vi + 73. (Calcutta: Indian Association for the Cultivation of Science, 1945.) n.p.

THE afterglow of nitrogen in a discharge tube was first observed in 1884, and attempts to explain it have been made at intervals for the last thirty-five years. In the first, and larger, part of this book the observations on active nitrogen during this period are reviewed. This part is well and stimulatingly written; the discussion of the work of Lord Rayleigh, Willey, Kaplan and others on the production, decay and chemical effects seems particularly well done and makes interesting reading; on the spectroscopic side Prof. Mitra seems on slightly less firm ground. He appears to have missed the work of Spealman and Rodebush showing that the yellow-green air afterglow is due to reaction between nitric oxide and atomic oxygen, not between nitrogen peroxide and ozone as stated in the book; but the bibliography on the nitrogen afterglow itself seems fairly complete.

Existing theories of the afterglow are briefly stated and receive some well-justified criticism. The second part of the book gives Prof. Mitra's new theory that the afterglow is due to recombination of ionized nitrogen molecules with free electrons. The case for this simple theory is put clearly; but it seems doubtful whether it can explain all the facts, and it is unlikely to receive general acceptance. The chief value of the book lies in its clear readable summary of experimental work.

A. G. GAYDON

GENETICAL ASPECTS OF SEED-GROWING*

By A. J. BATEMAN
John Innes Horticultural Institution

The Seed Trade at War

THE Second World War placed great difficulties before the British seed trade. Whereas Great Britain had previously relied overwhelmingly on imported seed, nearly all the usual channels were now closed. In 1941 seed firms were faced simultaneously with an almost complete drying up of their sources of seed supply and an increased demand for seed due to our ploughing-up campaign. A rapid expansion in the amount of seed-growing in Britain was imperative.

As a result of this development, the seed trade was brought face to face, more sharply than hitherto, with problems connected with the growing of seed under conditions in Britain. One of the most important of these problems was that concerned with the maintenance of varietal standards, which frequently showed a trend in the downward direction from the quality and yield of the same varieties when first brought into commerce. A major cause of this deterioration was genetical contamination, which might take place, either through mechanical admixture of seeds of different varieties during threshing, or through the admixture of pollen of different varieties during natural pollination of the growing crop. The former poses a mechanical problem which is relatively easy to solve because all the stages of threshing are under direct human control. The latter involves a more difficult problem as it concerns the behaviour of insect pollinators or of wind, neither of which can be put under direct human control. It was also one likely to be of particular importance in Britain, because for many crops the areas suitable for seed-growing were small, and spatial isolation was, therefore, likely to present difficulties. On the other hand, it was now easier to control the methods of seed-growing than when seed production had been carried on in several foreign countries.

This led to the introduction of recommended isolation distances for many crops, a policy which was carried further in some cases, where zoning schemes have been introduced. *Beta vulgaris* has several distinct cultivated sub-species, all completely interfertile—mangold, sugar-beet, garden beet and spinach beet. Each sub-species has now been given its own zone to prevent its being contaminated by any other sub-species. A similar zoning scheme has been introduced in Essex for *Brassica oleracea* sub-species.

Tolerance Limits to Contamination

The measures taken to achieve isolation should be related to the degree of isolation required. Reference to the large distances recommended in the accompanying table makes it appear that complete isolation is the aim. Though this may be attainable, the end is not necessarily worth the extreme precautions taken. For if the question is considered from a genetical point of view it will be seen that the maximum permissible contamination need not be zero, but will vary with the crop and with the use for which the seed is intended.

There are three uses to which such seed is usually put. First the seed may be sold for raising crops in the ordinary way (that is, not for seed production). This we will call commercial seed. Secondly, it may be kept by the seedsman for producing the commercial seed of later seasons. This is stock seed. Lastly, it may be used for continuing a highly selected line, consisting of only a few plants, from which stock seed is obtained. This is élite seed.

For commercial seed, contamination is only serious when it produces such a high proportion of plants not true to type that the commercial value of the crop is affected. In this instance the permissible amount of contamination should be of the same order of magnitude as the permissible amount of foreign seed. On the other hand, the tolerance-limit of contamination in stock seed must be much lower, for in later generations the hybrids would intercross with the whole strain and produce a widespread change in its genetical constitution. The tolerance-limit for élite seed must be lower still, for whereas if stock seed is spoilt it can be replaced by seed from the élite strain, the latter can only be replaced by the plant breeder and then only with much labour assisted by much good fortune. Even with élite seed there is a lower limit, set by the mutation-rate of the species, below which it is not worth trying to keep the contamination.

This last point brings us to the next, which is that it is unnecessary to attempt to reduce contamination

PARTS PRINTED IN ITALICS REPRESENT EXPERIMENTAL RESULTS; REMAINDER, GENERALLY PRACTISED RECOMMENDATIONS.

Crop	Isolation distance (in yards)	Authority	Year	Remarks
Brassica spp., in general	1760	Nat. Inst. Agric. Bot.[5]	1942	
	3520	Haskell[6]	1944	
Cauliflower	440	Haskell[6]	1944	
Cabbages and kale	1760	Haskell[6]	1944	
	26	*Tedin*[7]	*1931*	*Beyond this distance contamination remained 1%*
Turnip	880	Haskell[6]	1944	Customary in England
	220	Haskell[6]	1944	
Radish	*27*	*Crane and Mather*[8]	*1943*	
	500	Haskell[6]	1944	
Beets, etc.	1000	Haskell[9]	1943	
Spinach	440	Haskell[6]	1944	
Carrot	880	Haskell[6]	1944	
Cucumber	220	Haskell[6]	1944	
	1100	*Naghibin and Uzembaev*[2]	*1943*	*'Complete'*
Cotton	*16*	*Naghibin and Uzembaev*[2]	*1943*	*'Practically satisfactory'*
	55	*Nicolaisen*[10]	*1942*	
Tomato	*8*	*Currence and Jenkins*[11]	*1942*	
Pea	220	Haskell[6]	1944	'Complete'
Beans (runner beans)	*330*	*Nicolaisen*[10]	*1942*	
Beans (runner and dwarf)	nil	Haskell[6]	1944	Except in stock seed
	250	Haskell[6]	1944	Commercial seed
Onion	880	Haskell[9]	1943	In Canada
	1760	Haskell[6]	1944	Stock seed
Leek	250	Haskell[6]	1944	
Sorghum	*130*	*Hsu*[12]	*1934*	
	1760	Haskell[9]	1943	In Canada
	660	Salamov[12]	1940	U.S.S.R. regulation before 1940
	220–330	*Salamov*[12]	*1940*	*Salamov's recommendation*
Maize	220	Hayes and Immer[14]	1942	Minnesota Crop Improvement Association regulations.
	17	*Meijers*[15]	*1937*	*At this distance contamination 1 in 4,000*

* Adapted from part of a thesis approved for the degree of Ph.D. at the University of London.

below a level equivalent to the natural genetic variability of the variety. This applies even to élite strains. The natural variability is high in outbreeding crops like the species of *Brassica* which, for the same reason, are also most readily contaminated. It is low in inbreeding crops like wheat which are not readily contaminated. We, therefore, discern what is, for the seed-grower, a most convenient inverse correlation between the ill-effects on a species of a given level of contamination, and the chances of such a level being reached.

It is clear, then, that before deciding on the proper isolation requirements of a crop, we must first decide, in each instance, according to the breeding system of the crop and the purpose of the seed, what is the maximum contamination to be tolerated. Only then can we begin to consider what conditions must be observed to obtain the desired level of isolation.

Cryptic and Obvious Contamination

Before leaving the question of tolerance-limits for contamination a further word is required. At first glance it might appear that if isolation requirements have been based on the assumption that only complete isolation was satisfactory, the error has been on the safe side and nothing is required to improve matters but to relax the restrictions at present imposed on spatial isolation. The regulations have been designed, however, specially to prevent contamination between markedly distinct varieties, and while there may have been a tendency to exaggerate the dangers of such contamination, there has at the same time been an underestimation of the dangers of contamination which, not being between two very distinct varieties, is not of an immediately obvious nature. This is illustrated by the beet zoning scheme referred to above. For though no mangolds are allowed to be grown in the sugar-beet zone, little attention is paid to the isolation of sugar-beet varieties from one another.

When very distinct varieties are crossed, the hybrid is usually distinguishable from both parents. The very obviousness of that type of contamination is a safeguard, as it can be eliminated at once by roguing. (Roguing is the term used for the uprooting of 'rogues' or plants which are not true to varietal type.) The same applies if a variety homozygous recessive for a gene showing Mendelian dominance relationship is contaminated by one which is homozygous for the dominant allele. But contamination in the reverse direction would be invisible in the first generation and may be termed cryptic.

This contamination would only become apparent when segregation occurred in later generations, and might then be mistaken for contamination arising in the generation in which it appeared. Such a phenomenon might account for stories current in the 'mythology' of isolation, where contamination is asserted to have taken place when the nearest source of contamination was a mile or more away. An alternative explanation would be that cryptic contamination had occurred in a previous generation when the isolation distance had been much less, and was only now manifesting itself.

The elimination of the above kind of cryptic contamination after it had arisen would have to be by a laborious process of repeated roguing over several generations. Even this would not be effective if the parent varieties were distinguished by more than a few genes with major effects. But it is recognized that the varietal differences which are of value are generally due to large numbers of genes, each with small effect, so that roguing in any generation after the first is ineffective owing to an inextricable mixture of the genetic constitutions of the two varieties. This admixture usually produces a general deterioration in the qualities by which the variety was characterized. Roguing, in generations after the first, for any segregating major gene, would merely remove that part of the contamination closely linked with it.

To illustrate this point, let us assume that sugar-beet is contaminated by red beet. The F_1 has anthocyanin distributed throughout the plant, like its male parent. Roguing for red seedlings will effectively remove all contamination. Contamination of one sugar-beet variety by another, however, may be cryptic in either direction; but the subsequent diffusion of foreign genetic material throughout the variety is likely to be as profound in its effect on the variety's important features, such as sugar content and yield, as any contamination of an immediately obvious nature.

These considerations only apply, of course, when, after being exposed to contamination, the seed is to be grown for further generations, during which the foreign genetic material could diffuse. They do not apply, therefore, to commercial seed, where a larger amount of contamination, cryptic or obvious, is permissible. The seed-grower's reputation, however, is more likely to be affected by the amount of obvious, than by the amount of cryptic, contamination.

We may, therefore, distinguish between the isolation requirements of élite, stock and commercial seed. In élite seed the aim should be to keep cryptic contamination below a level comparable with the natural internal variability of the variety. Obvious contamination is not serious in this case as it can be counteracted by roguing. In stock seed one need be less rigorous in excluding cryptic contamination; but as roguing for obvious contamination would involve much work, this is less desirable in stock seed than in élite seed. In commercial seed cryptic contamination is relatively harmless, and though obvious contamination is no more serious agronomically it is psychologically more undesirable.

These differences in isolation requirements, apart from other considerations not discussed here, suggest the desirability of keeping the three stages of seed production as distinct processes.

Estimation of Isolation Requirements

As the matter stands at present, it would appear that the isolation distances recommended to seed-growers are based largely on conjecture and a desire to err on the safe side. This will be seen from the table, which shows isolation distances generally recommended together with, where available, isolation distances which have been shown to be adequate by experiments (in italics).

It will be seen that, in comparison with experimental evidence, the generally practised isolation distances are much too great.

At first sight this may not appear to be a defect; but, as these isolation distances are only concerned with the prevention of obvious contamination, and as the area available for seed growing is limited, excessive precautions against obvious contamination only reduce the possibility of obtaining adequate protection from cryptic contamination. It is, therefore, necessary to determine the minimum isolation requirements. Once known these could be applied to the prevention of both kinds of contamination.

Experimental evidence on this subject (see the table) is very sparse, and such as has been obtained has concentrated on the isolating effect of distance between varieties, and has tended to ignore other factors. Consequently inconsistencies have arisen, and in different experiments using the same crops over the same distances, different degrees of contamination have been obtained. Moreover, the results are often given in such a way that one cannot analyse the causes of the variation between experiments, or make any generalizations.

One of the factors often ignored in determining isolation is the number and arrangement of plants of both the variety open to contamination and the source of contamination. Though not the most striking examples of this omission (the latter must be given credit for considering the arrangement of the plants to be of sufficient interest to warrant recording) some papers on cotton by Russian workers serve to illustrate this point. Malinkowsky[1] grew several varieties together—one row of each. Contamination varied from 0 to 8·48 per cent with a mean of 4·63 per cent. It is not clear that all contamination was obvious for all the seed parents. Naghibin and Uzembaev[2] grew a strip of one variety in a field of another and observed 5·22 per cent crossing in rows adjacent to the strip. From the way in which contamination decreased with increasing distance from the strip they estimated that one kilometre would give 'complete isolation' though 15 metres gave 'practically satisfactory' results. Podyapolsky[3] grew two rows of a red variety, alternating with three rows of a green variety and observed 1·28 per cent crossing of red on to green and 40–58 per cent of green on to red.

The variability of results arising from such unstandardized experimentation has commonly led to data on contamination being accompanied by the qualification that the frequency of contamination is liable to vary from season to season, variety to variety, and place to place. Hence the practical seed-grower is unable to gain much assistance from such information.

In an effort to standardize methods of estimating contamination it has been recommended[4] that single plants of one variety should be surrounded by plants of another and that this should be carried out reciprocally. This would result in a statistic useful in the study of contamination, and might well be repeated for a wide range of pairs of varieties. Even such a single statistic, however, is actually an expression of the balance between two factors, the degree of self-fertilization taking place in one variety, and its crossability with the other. In a self-incompatible crop like turnip, for example, contamination would be 100 per cent in such a test; but in the field, where many plants of the same variety are together, self-pollen only represents a proportion of the intra-varietal pollen on a stigma, so that the degree of self-fertilization no longer has a direct influence on the amount of contamination. Such a statistic also does not allow for the effects due to isolation distance.

One must not, however, be too critical of such experimental evidence as does exist, for in view of the general lack of even the most empirical facts on this subject, it is most welcome.

Factors Affecting Isolation

In order to obtain experimental evidence which can be applied in practical seed-growing, it is necessary to consider all the factors likely to be involved in contamination and to plan experiments which will evaluate their respective influences.

The final causes of contamination are the proportion of the pollen on the stigmatic surface which is contaminant (excluding self-pollen in self-incompatible crops), and the relative compatibility of contaminant and non-contaminant pollen (again excluding self-pollen in self-incompatible crops). The latter will be characteristic of a given pair of varieties and will be little affected by environmental conditions. The former will be affected by a large number of factors involving both the plant and its environment.

(*a*) *Compatibility.*—Dealing first with the question of compatibility, we may say that self-incompatibility is of great importance in determining the liability of a crop to contamination. For especially in plants with anthers and stigma in a flower ripening at the same time and close together in space, a very large proportion of the intra-varietal pollen on a stigma will be self-pollen. Under these conditions self-compatibility will greatly reduce the chances of contamination as all the intra-varietal pollen on a stigma will be compatible. An example of the use of this relation was the selection in cotton[16] of lines in which self-pollen grew faster than pollen from other lines. This phenomenon (which might be termed hyper-self-compatibility) reduced the dangers of contamination. As might be expected, therefore, most of the crops in which contamination presents a serious problem are self-incompatible, for example, the *Brassica* species, radish, beet, carrot, parsnip.

Cross-incompatibility between varieties is the other factor affecting the relative compatibility of desirable and contaminant pollen. Within species it is unlikely to occur except between diploid and polyploid varieties.

In the *Brassica* genus, however, there are varying degrees of compatibility between species. For example, turnip (*B. rapa*) and Chinese cabbage (*B. chinensis*) are compatible, whereas cabbage (*B. oleracea*) and swede (*B. napobrassica*) are incompatible.

(*b*) *Pollen concentration.*—The proportion of contaminant pollen is, of course, dependent on the absolute amounts of both contaminant and non-contaminant kinds. The amount of non-contaminant pollen is a result of the degree of dichogamy, monœcy, or dioecy within the variety, the numbers of flowers per plant open at a time, the density of planting and the number of plants in the group. The amount of contaminant pollen is a result of the number of flowers per plant open at a time and the number of plants of the contaminant variety. It is also affected by the degree of isolation of the two varieties in space and time and the pollinating agents which bridge the spatial isolation. If the agents are insects, their methods of foraging are of importance. This has been shown[17] to vary with the ratio of the number of foraging insects to the nectar or pollen supply. It is likely to vary also with the species of insect. In self-pollinating plants which will set seed in the absence of insects the amount of contamination will be directly proportional to the amount of foraging. If wind is the pollinating agent its direction and speed, and the buoyancy of the pollen, will be factors. Doubtless the above list is an incomplete one of the number of variables affecting contamination.

Practical Applications

It is thus clear that contamination is a complex phenomenon. A number of the above factors are, however, outside the seed-grower's control. From the practical point of view one would first require to know

the crossability and degree of natural self-fertilization for a given pair of varieties. This would permit the estimation of the maximum amount of contamination likely to occur. Alternatively one could measure this directly by the method of Bryan and Marriott[4]. The next most important factors are isolation distance and the mass of both varieties. A study of the effects of these might well be combined with a study of the pollinating agent, with which the effects of both distance and mass are intimately connected.

Some preliminary work based on the above considerations has been proceeding at the John Innes Horticultural Institution in conjunction with the Agricultural Research Council during the past four years. Four crops were chosen—turnip, radish, beet and maize. The first three are self-incompatible and the last is monœcious with a low degree of selfing. The maximum contamination to be expected would in all cases be high (100 per cent in the first three crops). The first two are insect-pollinated, the last is wind-pollinated and the third is intermediate.

Using a number of experimental designs, for the four crops described, very consistent results were obtained as to the effects of isolation distance on contamination. In all cases contamination decreased rapidly at first and then more gradually down to approximately 1 per cent. When plotted on a graph, the results resembled a logarithmic curve of negative slope, the actual slope varying with the arrangement of the plants in the particular experiment. In the one experiment which involved isolation distances of up to 600 ft., it appeared that at distances more than 200 ft., increase in distance produced no detectable reduction in contamination. To draw practical conclusions from these experiments, we may state that for plots of moderate size the first fifty feet will reduce contamination to 1 per cent, but that the next fifty feet will produce scarcely any additional effect.

The effect of the mass of a variety in reducing its liability to contamination appears similar to that of distance, in that above a certain point further increase in size of plot is ineffective in reducing contamination. In radish this size corresponds to a strip twelve rows wide (at one foot spacing). These results agree well with the implications of observations made on the foraging habits of insect visitors or on pollen dispersal by wind, according to the pollination agent of the particular crop.

Though further work, using other crops and new planting arrangements, is required, the results at this stage (one paper has been published[5], the others will appear later) already give ground for confidence that this method of approach will yield information of practical value to the seed-grower. The geneticist will then be in the position where, given the maximum permissible contamination for a particular crop, he will be able to give authoritative advice on its isolation requirements.

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ESTABLISHMENT OF CYTOCHEMICAL TECHNIQUES

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Introduction

IN the latter half of the nineteenth century there developed a great wave of interest in cytology and histology. The experimental basis of this wave was the use of synthetic dyes for the staining of fixed sections. One of the main driving forces behind it was the desire to obtain information about the organisation of protoplasm, on both the physiological and chemical levels. On the physiological level many observations of great value were made; but on the chemical level very little of any value was obtained. The criticism of such shrewd observers as W. B. Hardy[1] and A. Fischer[2] made it plain that, with the usual techniques of fixation and staining with synthetic dyes, the results obtained were as much a function of the nature of the fixative, the physical characteristics of the fixed material, and of the technique of staining, as of the chemical and physical nature of the original protoplasm. Hence by the early years of this century the wave of interest in cytochemistry had lost much of its momentum, and was succeeded by a wave of interest in the experimental study of the living cell.

Experimental studies have been notably successful: in some instances cellular processes have been broken down into their physico-chemical components, and have become susceptible to mathematical description of an approximate type. But to-day a stage has been reached at which investigation is limited by lack of knowledge of cytochemistry. This limitation is imposed by the fact that many of the working hypotheses now entertained about cellular activity, though formulated as a result of studies of the living cell, involve assumptions of particular cytochemical organisations of protoplasm. This limitation is far-reaching, extending to fields as diverse as the mechanism of secretion, the mode of action of genes, ciliary and amœboid movement, and muscular contraction. In consequence there is a renewal of interest in cytochemistry.

Unfortunately many of the lessons to be drawn from the studies of earlier workers have escaped notice: this oversight has been attended by a lamentable failure to obtain rigorous and incontrovertible proof of the validity of many cytochemical techniques, some of which are in wide use. It is therefore proposed here to make a brief critical examination of seven cytochemical techniques, all of relatively recent origin. This examination has largely, though not exclusively, arisen from the facts which had to be considered in my recent investigation of techniques for the cytological demonstration of phosphatase[3].

(1) Centrifugal Stratification, etc.

The principle used here is to stratify a cell by centrifugation, then to divide the cell a number of times in the planes of the strata, and examine the distribution of substances in the various fragments so obtained. When this procedure is applied to some sea urchin eggs, it has been shown that the respiratory activity found in the fragments may substantially exceed the respiratory activity of the egg before it

was subjected to experiment[4]. This indicates that the cell has responded to the experimental conditions by synthetic activity or by a change of organisation: this type of response is to be anticipated as a source of difficulty in all such investigations, but has not been investigated in all cases. While this possibility exists, it is perilous to make deductions about the distribution of substances in the normal cell which are based only on the distribution of these substances in the fragments.

(2) Maceration Procedures

In these procedures cells are disintegrated by various techniques, and the resulting mush is subjected to differential centrifuging, by which means fractions of different densities are obtained. It is claimed that almost pure nuclei and mitochondria are found in certain fractions, and on occasion it is further claimed that the chemical substances, including enzymes, found in these fractions are both qualitatively and quantitatively identical with those present in the nuclei and mitochondria of the living cell. None of these claims has been established, and there are reasons for supposing that they are not justified. By microdissection methods Chambers and others have shown that when a nucleus is removed from a cell into a foreign environment, two things may occur: either the nucleus dissolves almost completely; or it sets into a rubbery gel. In neither case is its physical condition similar to that found in the living cell. Both the nucleus and cytoplasm must be regarded as highly complex colloidal systems, the nature of which, and the distribution of substances between which, are likely to be profoundly altered by much milder changes than those involved in maceration.

It therefore cannot be assumed that because an enzyme is found in such isolated 'nuclei' or 'mitochondria' this enzyme was in fact present in that structure in the living cell. The experimenter has the onus of finding whether the enzyme has moved from its original position. It is sometimes argued that identification of these cell fragments can be accomplished with the aid of staining reactions: the argument is that if part of a cell, and part of the debris obtained from macerated cells, have the same staining reaction, then the two things can be identified. But this argument is not in any way justified unless the staining is that secured by a highly specific chemical reaction, and is then only justified for the particular compound for which the reaction is specific. The association of other substances with this compound may be purely an artefact, and should be regarded as such until the contrary is established beyond doubt.

Even when a substance has been identified as forming part of a particular cellular system, its role may still be obscure. For example, an enzyme system may suffer either activation or inactivation, and its activity *in vitro* cannot be taken as more than an indication of its *in vivo* activity until the nature of its *in vivo* environment is established.

(3) Silver Nitrate—Acetic Acid Test for Ascorbic Acid

In this test a tissue is treated with silver nitrate in aqueous acetic acid. It is claimed that under these conditions ascorbic acid is the only substance likely to be present which will reduce silver: consequently any blackening of the tissue which results from this treatment is supposed to indicate the site of ascorbic acid.

Such a view does not take due cognizance of the fact that ascorbic acid and silver ion, and probably also their initial reaction products, are highly diffusible. As a consequence of these properties one would expect silver precipitation, if localized, to be in the mixing zone in the cell cortex if the reaction product is indiffusible, and on any suitable adsorbing body if the reaction product is diffusible. Accordingly observations made with this technique cannot possibly indicate the *in vivo* distribution of ascorbic acid unless: (*a*) the ascorbic acid is fixed chemically to an indiffusible body (that is, is not ascorbic acid but a derivative of that compound); and (*b*) the reaction product is indiffusible; or (*c*) the fixed ascorbic acid site has a very high affinity for the reaction product.

Whether these conditions do in fact prevail has yet to be determined.

(4) Calcium Phosphate Precipitation Test for Phosphatase

When a tissue section or squash, obtained by the use of a fixative which does not inactivate alkaline phosphatase, is incubated at *p*H 9·3 in the presence of glycerophosphate and calcium, phosphate ion is liberated from the glycerophosphate by the action of phosphatase in the preparation. This phosphate is precipitated in the section by the calcium present, and its presence may be revealed by several methods. It is claimed that these calcium phosphate deposits form an exact guide to the distribution of phosphatase in the preparation. Such a view cannot be sustained unless satisfactory answers can be given to three questions. These are: (*a*) How much phosphatase is destroyed by the experimental procedure; and if there is destruction, does it take place selectively at certain sites, or equally at all sites? (*b*) Is calcium phosphate in fact precipitated at sites of enzymic activity, or is it precipitated at other sites which have a specific affinity for calcium phosphate? (*c*) Is the phosphatase in a fixed preparation in its physiologically normal position?

For the technique used for alkaline phosphatase satisfactory answers have been given to these questions, but for other enzymes, for which analogous procedures are used, no such investigations have yet been made.

(5) Feulgen's Test for Deoxyribose Nucleic Acids

This well-known test is based on the fact that incubation of deoxyribose nucleic acids in *N* hydrochloric acid for ten minutes or more hydrolyses away part of the purine and pyrimidine residues, leaving a reactive sugar group which forms a highly coloured compound with reduced fuchsin. From the point of view of chemical specificity this test is probably adequate, but insufficient attention has been paid to the physico-chemical factors involved. As Stedman and Stedman[5] have emphasized, this treatment with *N* hydrochloric acid does more to nucleic acid than liberate reactive sugar groupings: it also depolymerizes nucleic acid, changing it from an indiffusible to a much more diffusible form. In consequence the reaction product between nucleic acid and reduced fuchsin is likely to be diffusible, and the staining of a section by Feulgen's method cannot be regarded as indicating the original position of nucleic acid until methods have been devised which will indicate the extent to which the experimental procedure has made nucleic acid and its reaction product with reduced fuchsin diffusible.

However, even if it should prove to be that by Feulgen's technique the normal site of nucleic acid is not revealed, the fact will remain that the sites which normally become stained under the influence of this reagent have a remarkably high affinity for the reduced fuchsin-nucleic acid compound, and elucidation of the significance of this fact can scarcely fail to be profitable. Therefore, while the cytochemical significance of Feulgen's test has yet to be defined, the numerous results obtained with it are not to be dismissed completely: they merely await a possible reinterpretation.

(6) Use of Absorption Spectra

The absorption spectrum of a purified substance present in a medium of known composition is often highly characteristic, but needs accurate determination before it is so characteristic. When a substance is present in a tissue its spectrum may be modified by interaction with neighbouring molecules, and masked by other molecular species absorbing in the same region. Determination of the cytological distribution of a substance from the absorption spectra of different parts of a cell is therefore hazardous. For example, chromosomes are suspected, but not yet proved, to consist of both proteins and deoxyribose nucleic acids. But so far no procedure has shown that a particular part of a chromosome has the *exact* absorption spectrum of any identified protein or nucleic acid. Until this *exact* identification can be made there is no justification for stating that absorption spectra have shown that the Feulgen positive bands of chromosomes contain large amounts of nucleic acid. The fact that these bands have overall absorption spectra which have a rather closer resemblance to that of some nucleic acids than to that of the proteins supposed (but not proved) to exist in chromosomes does not permit of any chemical conclusions.

(7) Tests with Enzymes

It is frequently claimed that when a tissue section, a squash, or a smear is treated with a purified enzyme, that part of the preparation will be dissolved away which is the natural substrate for the enzyme. Thus nucleases are supposed to remove nucleic acids, and proteases to remove proteins. Two objections must be raised to these conclusions.

First, it is hazardous to maintain that any preparation of an enzyme, however highly purified, is completely pure and specific for one substrate only.

Second, the fact that any part of a chromosome is removed by digestion with nuclease does not prove that this part is composed exclusively, or even mainly, of nucleic acid. Nor does failure to react with nuclease eliminate the possibility that the part is composed almost entirely of nucleic acid. If a part of a chromosome is composed almost entirely of protein, but is held together and attached to the rest of the chromosome by a matrix of nucleic acid, nuclease will destroy that part although it consists mainly of protein. On the other hand, if the part consisted almost entirely of nucleic acid, but were covered by a monolayer of protein, the nucleic acid might well be inaccessible to nuclease and therefore unattacked.

Conclusion

It is all too obvious from the foregoing remarks that cytochemical tests have been used without sufficient regard for criteria of proof, particularly from the physico-chemical aspect. The number of papers published involving the use of these unestablished techniques is enormous: for example, for each of the techniques (2), (6) and (7) there are many dozen examples, and for (5) the examples must be numbered in hundreds. All this work, with the exception of that carried out by technique (4), may prove from the chemical point of view to be completely vitiated. If this is so even for one of these techniques, an immense amount of careful and painstaking effort will have been wasted, simply as a result of ignoring the fact that cytochemical tests must be searchingly scrutinized from all three of the relevant points of view—biological, chemical and physical. This article then, though perforce critical, is not a plea for less cytochemical work, but a plea for exactness in cytochemistry.

[1] Hardy, W. B., *J. Physiol.*, **24**, 158 (1899).
[2] Fischer, A., "Fixierung, Färbung und Bau des Protoplasmas" (Jena, 1899).
[3] Danielli, J. F., *J. Exp. Biol.*, in the press.
[4] Shapiro, H., *Biological Symposia, Cold Spring Harbor*, **8**, 406 (1939).
[5] Stedman, E., and Stedman, E., *Nature*, **152**, 267 (1943).

SCIENCE AND RADIO

A CONFERENCE was held at Cambridge during May 18–19 on the subject of "Science and Radio". This conference was organised by the Cambridge branch of the Association of Scientific Workers and to it were invited certain members of the B.B.C. staff who are directly concerned with scientific topics in radio programmes.

The first day's session was mainly devoted to general considerations and programmes for unspecialized radio audiences; the second day's session to programmes for specialized audiences such as schools and other pre-selected groups. The first session opened with statements from B.B.C. representatives on the requirements and limitations of the medium.

Commander Ian Cox described the various types of radio programmes in which scientific material is normally projected, concentrating particularly on talks. He stressed, in this connexion, the essential difference between writing for reception by the ear and that for reception by the eye and how this difference has led to the opening up of a new technical field, operatives in which were described as 'producers'. Commander Cox also stressed the high degree of authenticity that can be attained if authoritative speakers will make themselves available to talk. He said, therefore, that it is only with the fullest co-operation from men of science that scientific material can be adequately projected to the radio audience. This applies not only to their willingness to speak, but also to their availability for advice and for providing up-to-date relevant information.

Mrs. Nesta Pain followed with a talk on feature programmes. The first criterion in choosing a subject for a radio feature, she said, is "Is it of dramatic value?" The feature is not simply a means of conveying information: it aims at giving more or less the same experience as a play. Some of the advantages of feature treatment are that it can convey the drama and excitement of discovery, can give a picture of the circumstances in which scientific work is carried out, and can portray character. The expert delivery of actors, changes in voice and situation all make for easier listening, which probably accounts for feature programmes having a larger audience than other forms of scientific programme. The greatest

attention is, however, paid to scientific accuracy at all stages in production.

Mrs. Mary Adams then gave a short consideration of science in television programmes. She pointed out the difference between the film and television as means of projecting science. She emphasized the difficulties in television of 'telescoping' lengthy experiments, but observed that this medium has the advantage of emphasizing the skill and personalities of demonstrators in cases where it is possible to show them at work. At present, she said, it is more effective for the apparatus necessary for scientific demonstration to be brought to the studio, but she hoped that within the next couple of years it would be possible to develop visits to museums and laboratories.

Dr. M. A. Clifford spoke on the subject of "Taking Biology to the Adult". He pointed out that in bringing science to the layman the approach by way of social interest is the most likely to be successful and that familiar experience (domestic or industrial) is the best starting point. He emphasized that the lecture is an art form, and said, in relation to adult education, that it is not necessarily desirable that the subject-matter of lecturers should be confined to their own particular field of research—the lecturer should share in some of the difficulties of his audience. The qualities required in a lecturer to laymen are not always found in the ranks of prominent men of science. In these remarks Dr. Clifford was, for the most part, referring to university extra-mural and W.E.A. lectures; and he suggested that the B.B.C. might profit from the experience gained in this field.

Prof. J. D. Bernal then followed, his subject being "A Policy". He started by saying that men of science themselves are largely to blame for the lack of scientific topics in news generally, and that there should be some scientific body responsible for disseminating information for those services that require it (for example, films, the Press and broadcasting). So far as the B.B.C. is concerned, he feels that too much responsibility devolves on individuals and there is obvious lack of co-ordination within the organisation. He then proceeded to develop a proposal that a scientific committee should be formed to advise the B.B.C., that its policy should be long-term and implemented by a staff of trained men of science. Its scope should include topical items as well as planned series. The fact that the B.B.C.'s charter is shortly to come up for review makes it an appropriate moment for such a proposal to be considered. Too many scientific men, in Prof. Bernal's view, regard the B.B.C. as something to listen to, and too few regard it as a service in which they themselves should be concerned to take part.

The second day's session was opened by Mr. Richard Palmer, who explained the scientific side of the B.B.C.'s services to schools. The general science broadcasts are intended for children of thirteen in modern or unreorganised schools, which often lack specialist teachers of science. Three basic themes provide much of the subject-matter in the general science series: science and human needs, human biology and the methods and social relations of the man of science. Most science broadcasts to schools are 'illustrated' with sound effects or by such radio devices as dramatization or present-tense commentary. Expert advice from specialists is constantly employed, although the experts themselves are not usually found to be the most suitable people to prepare radio-scripts.

Mr. Palmer was followed by Dr. A. Clow, who gave an account of the B.B.C.'s Services Educational Unit, formed in 1945 to broadcast educational programmes to the Forces. Science makes up one ninth of its total output. Dr. Clow said that the work of the Services Educational Unit is one of the most significant experiments ever made in sustained adult education. Apart from its reception by the Forces, the domestic audience is on occasion well over a million. So far as science is concerned the primary aim is not to attempt to turn listeners into men of science, but to establish a greater understanding of science and an improved knowledge about the activities of scientific workers and the work they do. Dr. Clow admitted that this might be criticized as a materialistic approach and one which overlooked the elegance of scientific method; but against that he urged that the sooner mankind realizes what science has done in the past two hundred years, and what it may do in the next two hundred, the better.

Miss Isa Benzie then outlined the B.B.C.'s work in the field of medicine and health. She emphasized that what has been achieved was essentially on the Corporation's initiative, and by now a service has been built up that is at the same time authoritative and personal. Radio has the advantage of combining impersonality, anonymity and confidentialness; the success of this was borne out by the burden of much correspondence received being: "Could you tell me on the wireless, because I don't like to ask my own doctor?" At the same time, one of the aims of the health broadcasts is to educate the public so that it will consult its doctor at the earliest signs of trouble.

Following on these contributions from members of the B.B.C. staff, Dr. A. P. Goffe of the London General Medical Branch of the Association of Scientific Workers then presented a paper on the broadcasting of medical science. Reasoning from the fact that the public is interested in the functions of the human body, in all categories of health work and medical science generally, Dr. Goffe called for definite times to be allotted weekly in broadcast programmes, and for an overall scheme to be planned on, say, a two-yearly basis. So far as planning is concerned, he proposed that, since it is the responsibility of men of science to communicate results of their work to the public, a medical advisory committee should be formed and that expert advice should be available at all times during the preparation of programmes. In general terms the aim should be to present in an ordered manner how the human body works, how disease comes about, the results of disease and how it may be combatted. Dr. Goffe then detailed a number of specific subjects that he feels should be included in such a series of programmes.

At the end of both sessions questions were asked and proposals made by members of the audience. In general these asked for more 'air-time' to be given to scientific topics and for a specialized broadcast service for scientific workers themselves, both at home and abroad. It was pointed out by the B.B.C. representatives in the latter connexion that the provision of specialized services for particular sections of the community is a difficult, if not impossible, matter at the present time in view of the very limited number of frequencies at the B.B.C.'s disposal, and the limited air-time available for transmitting all classes of material. They stated, however, that they are greatly encouraged by the recent increase in interest shown by men of science in radio, and hope that this will result in close liaisons and ready assistance rendered with a knowledge of the specific requirements of the medium.

OBITUARIES

General Georges Perrier

General Georges Perrier, veteran of the International Union of Geodesy and Geophysics, died on February 16, at the age of seventy-four; and so ended a brilliant century of geodesy, for General Perrier had followed in the footsteps of an equally distinguished father. General François Perrier (1833–88) was a contemporary and friend of Colonel Alexander Ross Clarke. It was he who, with the Spanish General Ibañez, carried the meridional arc which starts in the Shetland Islands across the Mediterranean to end in the heart of Africa. It was he also who, in 1870, organised the Service Géographique de l'armée as we knew it in the two World Wars, and who became the first director-general.

Georges Perrier was born in 1872. Twenty years later he did brilliantly at the École Polytechnique, and a wide choice of career was open to him. He preferred to follow his father's lead, joined the artillery, and in 1898 obtained his heart's desire and entered the Service Géographique. His earliest survey years were devoted to work in Africa; but in 1900, acting as organising French secretary, he arranged that meeting of the old Geodetic Association known as the Conférence de Paris. From that time onwards he was prominent in international geodetic co-operation. The next six years, 1900–6, were devoted to the revision of the famous arc of Peru. The later participation of the States of South America in the Union of Geodesy and Geophysics is largely due to the reputation and the friendships Perrier made in that continent. He then acted as commissioner on the boundary between the French and Spanish zones of Morocco, and afterwards (1913) on the much disputed northern and highland boundary of Albania.

If we, in England, already knew him well by name, it was in 1915 that we began to know him personally. At that time he was serving with a brigade of artillery in the Vosges, and our excellent system of sound-ranging owed much to the advice and demonstration he gave to a visiting British mission. In 1917 he commanded the 53rd Artillery Regiment. His brilliant war record is illustrated by seven mentions in dispatches, by Belgian, Czech and Italian decorations, and by the British D.S.O., in addition to the rank of commander in the Légion d'Honneur. Back in the Service Géographique, he had the disorganised geodetic surveys of France and her colonies to re-establish. Soon afterwards General Bourgeois, who had directed the Service Géographique during the War of 1914–18, and whose whole-hearted help had meant so much to Britain, retired; and here ill-luck met General Perrier for, by the accident of seniority, he was not to inherit the command of his father's creation. Instead, he returned to the artillery, to design and command its anti-aircraft branch. His military service ended in 1934 as divisional general and inspector general, becoming Grand Officer of the Légion d'Honneur.

Meanwhile, from 1919 onwards, and close in the councils of Schuster, Picard and Lallemand, he served as general secretary to the International Association of Geodesy. Perrier, one of those happy men whose work is their hobby, was tireless both in mind and body. Already in close touch with the geodesists of the world, he rapidly enrolled them in the new Association, and indeed, to the day of his death, it was through him, rather than direct to the Union, that they made their approach. He soon made his Association the court of geodetic appeal for all adhering nations. He started the *Bulletin Géodesique*, and the many other professional papers we owe to the Association. In 1926 he became a member of the Academy of Sciences, Paris, in 1929 professor of the École Polytechnique, and in 1935 member of the Bureau des Longitudes.

The loss of his two sons, in 1919 and 1939 respectively, was a grievous blow to him. Then came four years of war and enemy occupation. Nevertheless it is due to him that the Germans were able to get little of the knowledge, which they eagerly sought in his office, about the geodetic surveys of the occupied territories; and equally due to him that, during German occupation, money from Belgium, France and Holland found its way to sustain the offices of geodesy, seismology, vulcanology, and scientific hydrology.

From the initial meetings of the new Union at Brussels in 1919, to the meeting of the executive committee at Oxford in 1945, Perrier never missed a single gathering of the Union. His firm and steadfast character, his reliability, his singleness of mind and his unceasing energy will not be forgotten. When, at last, worn out by work and privation, he laid down his arms, his last words were of his work. In it he has left an enduring name.

H. S. L. W.

Academician Alexander Baikov

Academician Alexander Baikov, the eminent Russian metallurgist, died during April. His many and fruitful activities were bound up with Russian metallurgy, chemistry and metallography.

Baikov was a pupil of the great Russian chemist Mendeleyev and also of Le Chatelier, and introduced into metallurgy the methods of exact research and full experimentation. He had to a high degree the power of drawing practical conclusions from his observations.

Baikov's first scientific paper was on "An Investigation of Alloys of Copper and Antimony and Phenomena of Hardening to be Observed in Them". On becoming professor in the Polytechnic in Leningrad, he devoted himself to scientific work. At that period the developing industry of Russia was experiencing an acute shortage of qualified metallurgists, and Baikov did much to organise the training of suitable men; his school became known throughout Russia. In addition, he also worked in the laboratory of the Railway Institute, where he conducted research on rails and cements.

Baikov published an excellent textbook on "General Metallurgy" in which he dealt with the chief theories of the processes involved in smelting and the interaction of metals and slag.

Baikov combined his scientific work with a great deal of public activity. In 1910, a metallurgical society was organised of which he became the leading spirit. He became its first secretary and he frequently wrote theoretical articles on metallurgical questions for its journal. His scientific and public activities increased after the October Revolution of 1917. From its earliest days he was invited to work on the Supreme Council of National Economy.

He participated in the preparation and work of the first Congress of Russian Metallurgists, which took place in Leningrad in 1924, at which he read two papers on "The Theory and Practice of Pyritic

Smelting" and "Polymorphism of Iron and the Structure of Steel in Connexion with Röntgenographical Research". The latter paper proved of high importance in the new field of physical metallography.

As the years went by, Baikov's activities became ever more many-sided. At the request of the Baku Soviet, he investigated the causes of destruction of the concrete water-mains of the Shollarsk water supply; for many years he acted as consultant to the State Institute of Factory Planning; he edited the journal *Metallurgy* and was chairman of the Rails Committee of the Ministry of Railways. In 1927 he became consultant of the Soviet Institute of Metals. In 1932 Baikov was elected a member of the Academy of Sciences of the U.S.S.R.; in the same year he published his "Dialectics of Metallurgical Processes" in which he gave new interpretations of these intricate processes. In 1937 he was elected deputy for Leningrad to Supreme Soviet of the U.S.S.R. During the siege of Leningrad, despite many hardships, Baikov did not interrupt his work; he gave much of his strength in defence of the city. At the recent election Leningrad once more elected him a member of the Supreme Soviet; and as the oldest member he opened its first session. The Soviet Government expressed its high appreciation of Baikov's services by awarding him on three occasions the Order of Lenin and conferring on him the title of Hero of Socialist Labour.

V. GAVRILOV

NEWS and VIEWS

History of Science in the University of London: Prof. H. Dingle

PROF. H. DINGLE, who has been appointed to the University of London chair of the history and philosophy of science, amply possesses that first qualification required in a professor of the history and philosophy of science, namely, a real working knowledge of science. This he gained first at the Plymouth School of Science, Art and Technology and later at the Imperial College, South Kensington. There, after graduation, he was inducted into spectroscopic research by Prof. Alfred Fowler, whom he succeeded in the charge of the Sub-department of Astrophysics. He has produced an important treatise on the subject and has taken part in eclipse expeditions and in the international assemblies of astronomers. Perhaps it was the intense public interest in the astronomical confirmation of Einstein's general theory of relativity, after the War of 1914–18, that first awakened in Prof. Dingle the concern with the philosophy of science that has since grown ever stronger; his writings on relativity have continued from 1922 onwards. With the aid of a Rockefeller grant, he studied astrophysics and the philosophy of science in the United States, and returned there in 1936 to give the Lowell Lectures at Boston, Mass. His works in this field include "Science and Human Experiences", "Through Science to Philosophy", "Mechanical Physics", and many shorter contributions to collective works or philosophical journals. He has engaged in active controversies with Eddington and Milne on the adequacy of pure theory as a basis for science, and he is a well-known opponent of current views on the social relations and social planning of science. As a historian of science his work mainly lies in the future; besides his gifts of literary expression, he has wit, eloquence and power as a speaker.

Biochemistry at St. Thomas's Hospital Medical School: Prof. J. N. Davidson

IN recent years the recruitment of medically trained men as senior research workers in biochemistry has become somewhat infrequent, and it cannot be to the advantage of the general development of the subject that such a tendency should continue indefinitely. For this reason among others the recently announced appointment of Dr. J. N. Davidson to the chair of biochemistry at St. Thomas's Hospital Medical School is much to be welcomed. Dr. Davidson was trained first in chemistry and later in medicine, in both of which subjects he had a distinguished academic career at Edinburgh. He completed his scientific education by a year spent in the laboratory of Otto Warburg in Berlin. It was probably the latter experience which aroused Dr. Davidson's interest in enzymes and in cellular biochemistry; he has continued to work at both these subjects, and to the latter in particular he has made significant contributions. His histochemical investigations, carried out with Dr. C. Waymouth, of the role of nucleic acids in cell-growth and metabolism have added much to knowledge of this most fundamental branch of biochemistry. This work Dr. Davidson has done for the most part while holding lectureships in biochemistry, first at Dundee and later at Aberdeen; after a short interval at the National Institute for Medical Research, he now returns to a teaching post and to one which will give him a larger opportunity. From a man with his background and scientific achievement, we may confidently expect the development of a vigorous school of biochemical research.

Godman–Salvin Gold Medal of the British Ornithologists' Union: Award to Mr. P. R. Lowe, O.B.E.

THE British Ornithologists' Union has conferred the highest honour in its gift, the Godman–Salvin Gold Medal, to Mr. Percy R. Lowe for his researches in bird anatomy and his work for the preservation of birds. The Medal was instituted in 1919 as a memorial to two great ornithologists and original members of the Union, F. du Cane Godman and Osbert Salvin, to be awarded for outstanding work in ornithology by a member of the Union. Mr. Lowe's work on the anatomy of birds is generally admitted to be of the highest order, and though some may not have agreed with his conclusions, this has in no way diminished their regard for him as an outstanding anatomist. His studies in anatomy were commenced on his favourite group, the Charadriiformes, and he has contributed much towards the classification of these birds and their relation to each other as well as to other families and groups. His work on the ostriches and penguins was no less fruitful, and though his paper on the anatomy of the steamer duck evoked criticism from some American zoologists, this did not

deter the late Dr. Chapman from specially requesting Mr. Lowe to investigate the wing muscles of Gould's manakin, having regard to the complicated nuptial display of this bird.

Mr. Lowe has always taken a deep interest in bird preservation. His being the rare combination of man of science, all-round naturalist and keen sportsman, made him an undoubted leader in this field. To his balanced judgment and wide vision the British Section, International Committee for Bird Preservation, of which he is chairman, owes its success; and the regard with which he is held by other countries was evidenced by his election as chairman of the European Continental Section in 1938. The preservation of wildfowl has been Mr. Lowe's especial care, and it was owing to his activities that the International Conference on Wildfowl was held in London in 1927. He was mainly instrumental in forming the Wildfowl Inquiry Committee in 1936 which saw early fruit to its labours in the successful passage of the Wild Birds (Ducks and Geese) Protection Act 1939. His achievements for the preservation of wildfowl were recognized by the award of the Verner von Heidenstam Gold Medal by the Swedish Academy of Sciences in 1939.

Institute of Physics: Prof. J. A. Crowther

To mark their gratitude to Prof. J. A. Crowther for his fourteen years service as its honorary secretary, the past and present members of the Board of the Institute of Physics gave a lunch at Brown's Hotel, London, prior to the annual general meeting of the Institute on May 29. A testimonial was presented to him on behalf of the members of the Institute, recording their thanks for his unstinting service to the profession of physicist, and stating that his many actions and services "illuminate the pages of the Institute's records". During Prof. Crowther's tenure of office the Institute has not only grown in status and prestige, but also its membership roll has increased fourfold. The five branches in Great Britain and the four subject groups dealing with special branches of applied physics have all been established during this time. Among other new activities for which Prof. Crowther was largely responsible should be mentioned: the establishment of certificates in laboratory arts, and of national certificates in applied physics, the series of conferences and exhibitions and summer schools on various aspects of industrial physics, and the preparation of reports on "The Education and Training of Physicists" and on "The Teaching of Mathematics to Physicists". During the War, Prof. Crowther did much to ensure that the services of physicists were utilized to the best advantage, and he has continued this important work during the resettlement period. He has represented the Institute on many outside bodies, including the Parliamentary and Scientific Committee, of which he was vice-chairman. Prof. Crowther has now been elected a vice-president of the Institute of Physics, so that it will still be able to draw on his great experience and knowledge of physicists and physics. It will be recalled that Prof. Crowther is also retiring from the chair of physics at the University of Reading which he has occupied since 1924 (see *Nature*, March 30, p. 401).

Institute of Physics

At the annual general meeting of the Institute of Physics held on May 29, the following were elected to take office on October 1: *President*, Prof. A. M. Tyndall; *Vice-Presidents*, Prof. J. A. Crowther, Dr. H. Lowery and Mr. A. J. Philpot; *Honorary Treasurer*, Mr. E. R. Davies, *Honorary Secretary*, Dr. B. P. Dudding; *New Ordinary Members of the Board*, Dr. A. McCance, Mr. W. S. Vernon, Dr. F. A. Vick. Dr. G. F. C. Searle was elected an honorary fellow of the Institute.

The report for 1945 of the Institute shows that membership increased by 307, the highest since its foundation in 1918. Early in the year, at the invitation of the Ministry of Education, the Institute accepted the functions assigned to it by the Ministry in connexion with the arrangements for the award of national certificates in applied physics. The Ministry has now issued rules for the scheme and "Notes for the Guidance of Colleges and Schools". Mr. N. Clarke has been appointed to the new post of assistant secretary of the Institute. The report records that the attendance at the conference on "Instruments for the Automatic Controlling and Recording of Chemical and Other Processes", held jointly with the Institution of Chemical Engineers and the Society of Chemical Industry in October, totalled five hundred. The Institute also took an active part in the celebrations of the fiftieth anniversary of the discovery of X-rays. The Australian branch is still active, and the five local branches and three subject groups in Britain held forty-three meetings (some jointly) during the year. Other matters considered by the Board during the past year included "The Code of Practice in Respect of Consulting and Similar Work accepted by Members of Whole-time Academic Staff", prepared by the Joint Council of Professional Scientists, the release from war-time posts and reallocation of scientific men and the index to X-ray diffraction photographs.

Chemical Engineering at Cambridge: Prof. T. R. C. Fox

Early in 1945 the University of Cambridge received from the Shell Group of Oil Companies an offer to endow in the University a chair of chemical engineering. This offer was accepted by the University on March 2, 1945. It has now been announced that Mr. Terence Robert Corelli Fox, of King's College, Cambridge, has been elected into the professorship. Mr. Fox, who was born in 1912, was educated at Regent Street Polytechnic Secondary School and entered Jesus College, Cambridge, in 1930 with a scholarship in natural science. He read engineering and obtained a first class in the Mechanical Sciences Tripos in 1933. During 1933–37 he was employed in Imperial Chemical Industries, Ltd., as technical assistant to the chief engineer at Billingham. In 1937 Mr. Fox returned to Cambridge as a demonstrator in the Engineering Laboratory. He was elected a fellow of King's College in 1941 and was made a temporary University lecturer in 1944.

Ministry of Food: Scientific Advisers

Sir Jack Drummond, scientific adviser to the Ministry of Food, and Dr. T. Moran, director of research and deputy scientific adviser, have resigned their appointments with effect from May 31. Sir Jack Drummond is taking up the appointment, previously announced, as director of research to Messrs. Boots Pure Drug Co., Ltd., Nottingham (see *Nature*, **155**, 603; 1945). Dr. Moran is returning to his post as director of the Cereals Research Station, St. Albans, in charge of the laboratories of the

Research Association of British Flour Millers. Pending the appointment of a successor to Sir Jack Drummond, Dr. M. Pyke, who has returned to the Ministry's service from the Control Commission for Austria, will take charge of nutritional matters, and Dr. H. R. Barnell will deal with technological problems in regard to particular commodities.

British Association: Forthcoming Meetings

THE British Association will hold its annual meeting and two conferences during July. As the Association has no meeting-rooms of its own, various societies have generously granted the use of their premises for these occasions. At the annual meeting, to be held in the hall of the British Medical Association on July 20, the president, Sir Richard Gregory, will deliver his address on "Civilization and the Pursuit of Knowledge". Empire and foreign scientific delegates will be entertained at luncheon, and there will be a soirée in the rooms of the Geological Society. On the following day, July 21, the Association will hold an afternoon reception at Down House, which has already been re-opened to the public since the War. On July 8, a conference will take place in the theatre of the Royal Institution, on the "Dissemination of Scientific Information to the Public, through the Press, Broadcasting, Films, Museums and Exhibits". This conference will be held by the Association in collaboration with the Royal Society and the Empire Scientific Conference. Sir Henry Dale will preside. On July 25, the Association is arranging a conference to discuss the relation of the United Nations Educational, Scientific and Cultural Organisation to the universities, to be held at the Royal Institute of British Architects. There will be sessions on the intra-mural and extra-mural work of universities, and on their international relations. Among the speakers will be Sir Richard Gregory, Sir Alfred Zimmern, Mrs. Layton, Miss Lynda Grier, Dr. Maxwell Garnett, Prof. Bonamy Dobrée, Mr. P. R. Morris and Dr. Julian Huxley.

While this year's annual meeting of the British Association will be exceptional in being held in London on one day only, the pre-war practice of meetings lasting a week and held in provincial centres will be resumed in future. The meeting in 1947 will take place in Dundee, where the meeting in 1939 was cut short by the imminence of war. In 1948 the Association will meet in Brighton.

Handbooks of the British Fauna and Flora

A CONFERENCE convened by the Association for the Study of Systematics in Relation to General Biology was held in the rooms of the Linnean Society on May 30 to consider the present position regarding the production of handbooks on the British fauna and flora. Representatives of fifteen biological societies and organisations, whose programmes include this type of work, were present. In his opening remarks, Dr. W. B. Turrill pointed out that the matter had been under review by the Association before the War and that the way had been cleared by the publication of its "Bibliography of Key Works for the Identification of the British Fauna and Flora". There are many groups of animals of which there are neither monographs nor handbooks in existence, and it is most desirable that the existing gaps should be filled. The position with regard to plants is more satisfactory since, although some groups of cryptogams have no monographs or handbooks and others need revision, the phanerogams are already adequately covered. Capt. C. Diver outlined the work already done and stated that a list, based on the "Key Works", has been prepared to show these gaps. A measure of co-ordination between interested publishing bodies is necessary to obtain some degree of uniformity in treatment and arrangement. Considerable discussion followed during which it became evident that there is a general desire for a complete new series forming a "Fauna et Flora Britannica". The difficulty of finding authors able and willing to write handbooks for certain groups was discussed, as well as the means of financing publication. By the general wish of the meeting, the Association was urged to prepare a scheme, including recommendations regarding scope, format, groups most urgently needing attention and finance, for submission to a similar conference to decide on a line of action.

Conference on Industrial Spectroscopy

THE newly formed Industrial Spectroscopic Group of the Institute of Physics will hold its first conference during July 5–6 at the Wellcome Research Foundation, 183 Euston Road, London, N.W.1. The Conference will review the present status of industrial spectroscopy and will be open to all interested without charge, whether members of the Institute or not. The meetings on July 5 (2.30–7.0 p.m.) will be devoted to infra-red absorption spectroscopy. The speakers will be Dr. H. W. Thompson, of St. John's College, Oxford, on the significance of absorption spectroscopy for analytical purposes, and Mr. G. F. Lothian, of the South-West Essex Technical College, on equipment for infra-red absorption measurements. The meetings on July 6 (10.0 a.m.–5.0 p.m.) will deal only with emission spectroscopy. Mr. Nickelson, of Woolwich Arsenal, will discuss equipment for spectrographic analysis; Mr. A. Walsh, of the British Non-Ferrous Metals Research Association, will speak on spectroscopic light-sources for the analysis of metals and alloys; Mr. H. T. Shirley, of the Brown-Firth Research Laboratories, on a statistical examination of sources of error in the spectrographic analysis of alloy steel.

Exhibition of Early Medical Manuscripts and Books at Edinburgh

A SELECTION of books and manuscripts, to illustrate a series of lectures by Dr. Douglas Guthrie on the history of medicine, has been placed on view in the Upper Library Hall of the University of Edinburgh. Included among manuscripts of the thirteenth century are beautifully written copies of the "Breviarium Medicum" of Serapion, a Syrian physician of the ninth century, and of Avicenna's "Canon of Medicine", a favourite text-book, by the "Prince of Arabian Physicians", which retained its popularity for many centuries and is said to be used still by native practitioners in India. There is also a fifteenth century manuscript of the "Regimen of Health of Salerno", the best-known work of that famous school of medicine. It contains a full-page illustration of a medieval physician and one of his patients. A modern manuscript of much interest, on loan from the Royal College of Surgeons of Edinburgh, is a volume of notes of lectures on surgery, delivered by Lister while he was at Glasgow, containing the first written account of his "antiseptic principle". Among the printed books the greatest rarity is "Christianismi Restitutio", published in 1553 by Michael Servetus,

who suffered death by burning at the hands of Calvin. Although it is a theological work, it contains an account of the pulmonary circulation. Only two other copies of the book are known to exist. There are also on view first editions of "De Re Medicina" by Celsus, dated 1478; Vesalius' "Fabrica", 1543; Harvey's "De Motu Cordis", 1623 (from the library of Alexander Monro *tertius*); Willis's "Cerebri Anatome", with illustrations by Sir Christopher Wren; Jenner's "Inquiry into the Causes, etc., of Cowpox", 1798; and of W. T. G. Morton's brochure on the use of ether as an anæsthetic, 1847. One of the show-cases contains a collection of illustrated herbals, and a catalogue, dated 1683, of the Physic Garden, Edinburgh, which occupied part of the present site of Waverley Station. The exhibition is remaining open, during library hours, until the end of June.

Geographical Research Memoirs

At a recent meeting, the Council of the Royal Geographical Society decided to inaugurate a new Geographical Research Series of Special Publications. The Council's object is to facilitate the publication of scientific memoirs on any aspect of geography, too long or too specialized for publication in the *Geographical Journal*. The cost of publication will be met by the Society. Each memoir, in style and format similar to the *Geographical Journal*, will not normally exceed sixty-four pages of print, and allowance has been made in estimating cost for a limited number of plates, maps, and figures in the text. The distribution will be about five hundred copies. While papers offered for publication are normally expected to be the products of mature scholarship, theses for a doctorate or similar research degree from post-graduate students will also be considered. Papers for this series should be sent to the Secretary, who will submit them for consideration by the Publications Committee of the Society.

Manchester Museum

In spite of staffing and storage difficulties, it is stated in the report for the year 1944–45 of the Manchester Museum that departmental routine work has been progressive. Several collections stored away from the Museum during the War have now been returned and are again on exhibition. An experiment which will be of interest to other museums developing school services was the loan of a quantity of Australian material to a school which had been studying that country. The specimens (minerals, plants, insects, mammals, etc.) were arranged by the pupils themselves as an end-of-term exhibition to illustrate the class-work covered. Another interesting scheme was organised by the Extra-Mural Department of the University of Manchester. This took the form of "One-day Schools" for men and women of the Royal Army Ordnance Corps, who spent whole days in the Museum, where they were introduced to a number of scientific hobbies and told how the Museum could help them if they became interested in any one of them. Talks and discussions with members of the Museum staff took place in the class-rooms, and these were followed by inspection of the various collections in the galleries appropriate to the hobbies. The success of the scheme suggests that further work along these lines might be usefully developed. A short list of publications, a list of museum lectures given during the year, and a list of acquisitions are appended to the report.

Linnean Society of London

At the anniversary meeting of the Linnean Society of London held on May 24, Prof. G. R. de Beer was elected president for the period 1946–47. The following elections also took place: *New Members of Council*, Prof. G. R. de Beer, Dr. R. Dennell, Dr. A. T. Hopwood, Mr. H. W. Parker and Dr. W. B. Turrill; *Treasurer*, Colonel F. C. Stern; *Secretaries*, Dr. Malcolm Smith (zoology), Dr. B. Barnes (botany). Forty new fellows were elected.

The following foreign members were also elected: Prof. Jan Bělehrádek, rector of the University of Prague; Prof. H. Boschma, director, Natural History Museum, Leyden; Dr. Josias Braun-Blanquet; Prof. Edouard Chatton; Dr. François Gagnepain; Prof. Johan Hjort, president, International Council for the Exploration of the Sea; Dr. N. A. Maximov, director, Institute of Plant Physiology, Moscow; Knud Hensch Stephensen, Zoological Museum, University, Copenhagen; Dr. Victor van Straelen, director, Natural History Museum, Brussels; Prof. Johanna Westerdijk, Centraalbureau voor Schimmelcultures, Baarn, Netherlands.

Mr. Frederick J. Pittock was elected an associate *honoris causa*.

In his presidential address, after making some remarks on the welfare of the Society, Mr. A. D. Cotton, the retiring president, made some further observations on his studies of "Tree Senecios of Africa".

Royal Microscopical Society

The following have been elected officers of the Royal Microscopical Society for 1946–47: *President*, Dr. James A. Murray; *Vice-Presidents*, Mr. J. E. Barnard, Dr. G. M. Findlay, Mr. C. F. Hill, Dr. T. E. Wallis; *Hon. Treasurer*, Mr. S. R. Wycherley; *Hon. Secretaries*, Dr. R. J. Ludford, Dr. H. Moore; *Ordinary Members of Council*, Mr. F. D. Armitage, Dr. L. P. Clarke, Dr. F. Greenshields, Mr. F. C. Grigg, Mr. N. Ingram Hendey, Major Maxwell Knight, Dr. J. E. McCartney, Mr. Chetwynd Palmer, Mr. R. Ross, Mr. D. J. Scourfield, Mr. E. Wilfred Taylor, Mr. J. M. Watson; *Hon. Editor*, Dr. G. M. Findlay; *Hon. Curator of Instruments*, Mr. Chetwynd Palmer; *Hon. Curator of Slides*, Mr. N. Ingram Hendey; *Assistant Secretary and Librarian*, Mr. J. W. Kettlewell.

Royal Society of South Africa

The following have been elected officers of the Royal Society of South Africa for 1946: *President*, Prof. R. S. Adamson; *Secretary*, Mr. A. J. H. Goodwin; *Treasurer*, Prof. R. W. James; *Librarian*, Prof. E. Newbery; *Editor of Transactions*, Dr. A. L. du Toit; *Members of Council*, Prof. D. Burnett, Prof. P. J. du Toit, Prof. B. L. Goodlet, Prof. A. P. Goossens, Dr. J. Jackson, Mr. J. G. Rose, Prof. I. Schapera, Prof. B. F. J. Schonland, Prof. F. Walker and Dr. C. L. Wicht.

Announcements

Dr. R. H. Barfield, a principal scientific officer of the Radio Division, National Physical Laboratory, has been appointed to the research and development staff of Wild-Barfield Electric Furnaces, Ltd.

A series of four lectures on "Problems of Modern Biology in Relation to Atomic Physics" will be delivered by Dr. Max Delbrück, of Vanderbilt University, Tennessee, in the University of Manchester on June 11, 12, 13 and 14.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Electron Micrographs of Molecules on the Face of a Crystal

EVAPORATION of aqueous solutions of purified plant viruses on the collodion-covered screens used for electron microscopy leaves many of their elementary particles in a regular array[1]. With very dilute solutions this regularity is largely two-dimensional, but more concentrated solutions sometimes furnish the kind of three-dimensional order characteristic of a true crystal. Such regularity has been imperfect when these crystalline masses were thin; and clear electron micrographs have not been produced from thicker masses, or from single crystals, because of the diffuse electron scattering by their underlying molecular layers.

The distribution of macromolecules on the faces of individual crystals can, however, be reproduced through the shadowed replicas already described[2]. To prepare one of these, a suspension of small crystals has been smeared on a clean microscope slide and allowed to dry in the air. It has then been placed in a vacuum chamber and coated with a very thin layer (*c.* 8 A.) of metallic gold evaporated from such an angle that objects cast shadows five times their heights. The resulting ultra-thin gold film has been backed-up with a thin collodion layer and the two together have been floated on to a water surface and picked up on specimen screens. The most serious difficulty encountered in taking a satisfactory replica of individual virus crystals has been caused by the strong salt solutions in which they have been formed and preserved. The crystals with which we have dealt have been in half-saturated magnesium sulphate, and it is inevitable that this large amount of foreign salt should tend to smear over and hide much important detail. It has often interfered with the replication of molecules and even of the crystalline outlines; but where detail at the molecular level has broken through the overlying salt, the intra-crystalline configurations have been clear.

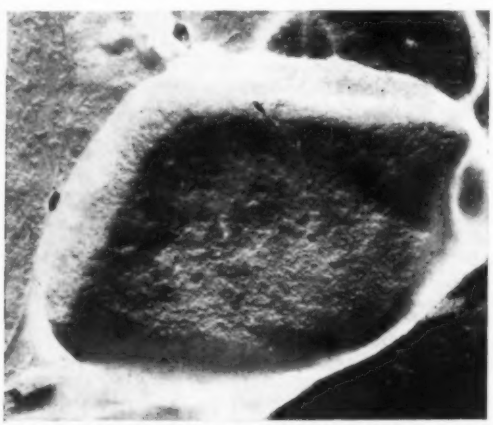


Fig. 1. ELECTRON MICROGRAPH OF A SHADOWED REPLICA OF A SINGLE CRYSTAL OF THE BEAN MOSAIC VIRUS. MAGNIFICATION 8,200

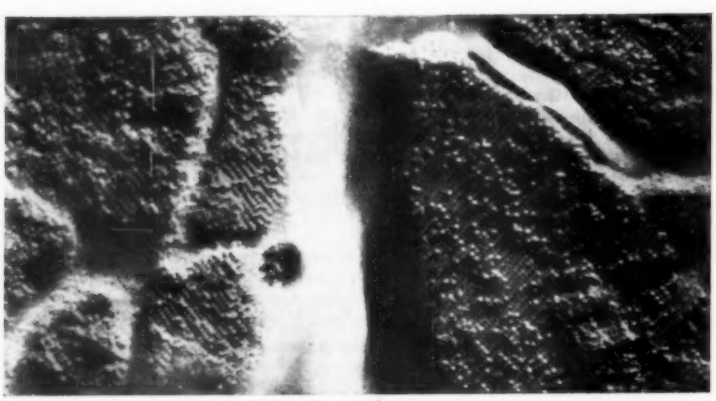


Fig. 2. A MICROGRAPH OF PARTS OF TWO CRYSTAL FACES SHOWING MOLECULAR ARRAY. MAGNIFICATION 26,000

Fig. 1 is an electron micrograph of a replica of a single crysta of the southern bean mosaic virus. The regular arrangement of its molecular particles can be discerned even at this low magnification. The faces of two adjacent crystals that appear at a higher magnification in Fig. 2 consist for the most part of a rectangular network of apparently flattened particles.

Photographs from faces of various crystal forms show the different geometrical arrangements of their molecules. Evidently such a set of pictures is the basis for a direct means of establishing, and in fact seeing, the structure of a crystal. The southern bean mosaic virus crystals are now being analysed through these and other similar micrographs. At the same time, this essentially new method of crystal structure determination is being further developed, so that it can be applied to protein and other macromolecular substances which are hard to investigate by the classical methods of X-ray diffraction.

W. C. PRICE
(Univ. of Pittsburgh, Pittsburgh, Pa.)
RALPH W. G. WYCKOFF

National Institute of Health,
Bethesda, Md.
April 16.

[1] Price, W. C., Williams, R. C., and Wyckoff, R. W. G., *Science*, **102**, 277 (1945); *Arch. Biochem.*, **9**, 175 (1946).
[2] Williams, R. C., and Wyckoff, R. W. G., *Nature*, **156**, 68 (1945).

Complete and Incomplete Crystals

GIVEN the molecular volume of a substance and the volume of its elementary cell, the Avogadro number N can be calculated. With ideally perfect crystals, the result of this calculation will be the exact value of N.

Since 1919 calcite has been used for standard X-ray measurements instead of sodium chloride, because sodium chloride does not give sharp reflexion lines. A series of substances has since been investigated with respect to the degree of sharpness of this reflexion[1]. The imperfectness was explained by assuming a mosaic structure of the crystals. In reality not only mosaic structure but also holes in a lattice and ions at interstitial places contribute to make a crystal irregular. It is proposed to describe such a crystal as 'incomplete'.

On the other hand, the degree of hardness of the materials investigated is also connected with the incompleteness of crystals.

It will be seen from the above-mentioned considerations that the value of the lattice constant (a) and especially the density (d) of the solid will be affected. As a matter of fact, in most cases the value of N found from the formula:

$$N = c.M./a^3.d,$$

where M is molecular weight, and c is a factor related to the crystalline structure of the solid, is too high owing to the value of d (experimental) being too low.

Materials	$N \times 10^{-23}$	Degree of incompleteness
Diamond	6·0227* ± 0·0003	0 per cent
Alkali chlorides (from solution)	*6·0228	0·00[4] „
Quartz	6·0229 ± 0·0006	0·00[3] „
Calcite	*6·0230 ± 0·0014	0·00[4] „
Selenium	*6·0235 ± 0·0010	0·01[2] „
Alkali chlorides (from melt)	*6·0237	0·01[4] „
Lead monoxide (red modification)	6·0253 ± 0·0027	0·04[2] „
Tin	6·0260 ± 0·0010	0·05[4] „
Various metals, average	6·0263	0·05[5] „
Lead monoxide (yellow modification)	6·0320 ± 0·0035	0·15[3] „

* Values marked thus are wholly or partially derived from measurements, to be given fully elsewhere, made in this Laboratory.

In the accompanying table different values of N are given as found by investigating the materials mentioned: the degree of incompleteness, expressed in percentages, is also given, diamond being considered as a complete crystal.

Attention is directed to the following points:

(1) The best value of N, given by Birge (1941), taking calcite as a standard, is $6{\cdot}0228^3 \pm 0{\cdot}0011 \times 10^{23}$ mol./gm.mol.

(2) Crystals of diamond, the alkali chlorides from solution, quartz and calcite are relatively complete.

(3) Metals, in accordance with their mosaic structure, show a high value of N.

(4) Crystals of the alkali chlorides prepared by melting (Kyropoulos monocrystals) are incomplete compared with those from a solution.

N. W. H. ADDINK

Natuurkundig
Laboratorium,
N. V. Philips'
Gloeilampenfabrieken,
Eindhoven.
Feb. 27.

[1] Several articles in *Z. Krist.*, 89 (1934).

Structure of the So-called *cis* Decalin

THE structures of the two stereoisomeric hydrogenation products of naphthalene and their relationship to cyclohexane were discussed by one of us in 1943 [1]. It was pointed out that the *trans* structure proposed by Mohr [2] (Fig. 1) will probably be correct, but reasons were given indicating that Mohr's *cis* structure containing two cyclohexane rings of the 'cradle' form should be rejected in favour of another structure shown in Fig. 1*b*. The carbon skeleton of both these two molecules is found in the well-known diamond lattice. Using the notation introduced in a paper dealing with the cyclohexane problem [3], the two cyclohexane rings of *trans*-decalin may be said to share two $\varkappa$-bonds; in *cis*-decalin, however, one $\varkappa$-bond and one ε-bond.

The correctness of these assumptions has been proved, using the electron diffraction technique and methods of computation worked out in this laboratory and based on the rotating sector method with subsequent Fourier analysis of the molecular scattering curves obtained from microphotometer records of the diffraction diagrams. The results are therefore entirely uninfluenced by physiological effects occurring when visual methods are used.

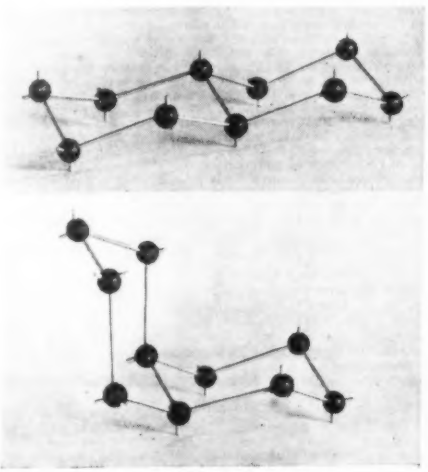


Fig. 1. STRUCTURE OF *cis*-DECALIN: (*a*) ACCORDING TO MOHR; (*b*) ACCORDING TO BASTIANSEN AND HASSEL

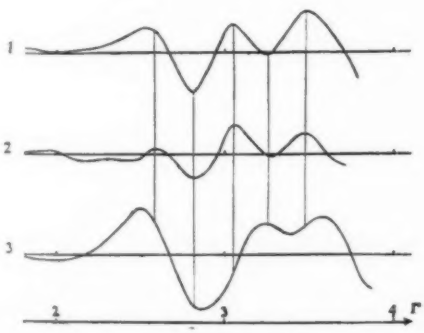


FIG. 2

The two $\sigma(r)/r$ curves giving the intramolecular distribution of nuclear distances in the two isomers show a high degree of conformity. We therefore preferred to discuss the differential curve obtained by subtracting the experimental $\sigma(r)/r$ curve of the *trans* form from that of the *cis* form. We then compared this curve with theoretical curves corresponding to our $\varkappa$-ε model (Fig. 1*b*) of *cis*-decalin and the structure proposed by Mohr. The interval between $r = 2$ and $r = 4$ A. would be expected to include the most pronounced differences in internuclear distances, and the three differential curves reproduced in Fig. 2 are: (1) theoretical curve for the *cis* ($-\varkappa\varepsilon$) structure of Fig. 1*b*, (2) experimental curve, and (3) theoretical curve for Mohr's *cis* structure, all related to the interval mentioned.

A comparison of experimental and theoretical curves seems to settle beyond doubt that *cis*-decalin has the $\varkappa$-ε structure given in Fig. 1*b*. The only symmetry element being a two-fold axis of symmetry, the *cis*-decalin should exist in two optically active forms.

A more detailed report will soon be published in *Tidsskrift for Kjemi, Bergvesen og Metallurgi* (Oslo).

O. BASTIANSEN
O. HASSEL

Chemical Institute.
University, Oslo. April 13.

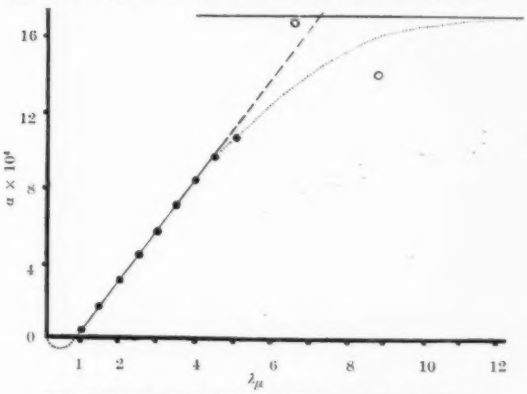
[1] Hassel, O., *Tid. Kjemi, Bergv., Met.*, **3**, 91 (1943).
[2] Mohr, E., *Erdm. J. Chem.*, **98**, 315 (1918).
[3] Hassel, O., *Tid. Kjemi, Bergv., Met.*, **3**, 32 (1943).
[4] Finbak, C., *Avh. Norske Vid.-Akad., Oslo, Mat. Nat. Sci. Kl.*, No. 7 (1941).

Infra-Red Emissivity of Metals at High Temperatures

A SPECIALLY designed vacuum-tight, water-cooled furnace constructed of silica and steel has been used to measure the emissivity of various metals at temperatures of 1,000°–1,500° C. and wave-lengths 1·0–4·5 μ.

The specimen, formed from a thin sheet bent into the form of a cylinder, was resistance-heated by a current of 300 amp. The intensity of the radiation from the inside and the outside of the cylinder was measured by means of a rock-salt prism spectrometer and a thermopile, and the ratio of these intensities after correction gave the emissivity. The radiation from the inside proceeded through the slit formed by leaving the two edges of the sheet specimen just a little apart when bent, so forming a slightly incomplete cylinder. The black-body so formed was not perfect and was therefore calibrated by using a specially constructed platinum sphere black-body against which it could be compared. Results were obtained for pure iron and for platinum to an accuracy of about 2 per cent.

Since the emissivity at room temperature can be found by applying Kirchhoff's law to reflectivity measurements, it is possible to deduce the temperature coefficient of emissivity in the near infra-red. This is of some importance, since it has been known for some time that a transition region must occur between the far infra-red region (where the Hagen–Rubens law predicts a coefficient equal to half the temperature coefficient of resistivity) and the visible region where the temperature variation is extremely small, and, in some cases, in the opposite direction to that predicted from electro-magnetic theory. A careful study of the literature has shown that in a number of instances results have been found indicating that, for a wave-length specific for each metal, the emissivity and reflectivity are constant over an extremely wide range of temperature. The results for tungsten given by the late Prof. Ornstein [1] furnish the outstanding example of this constancy.



TEMPERATURE COEFFICIENT OF EMISSIVITY FOR PLATINUM
Horizontal line, Hagen–Rubens theoretical limit; o, Hagen–Rubens experimental points; ●, present experimental results.

The accompanying graph, based on the present work, shows the transition region for the temperature coefficient of emissivity of platinum and indicates on extrapolation an X-point (wave-length of constancy) at about 0·93 μ. Results for pure iron give a similar straight line showing a well-defined X-point at about 1·0–1·5 μ (the uncertainty is due to inaccuracies in the available data for room temperature reflectivity). The origin of this X-point is as yet unknown. Work is at present in progress on molybdenum and nickel.

An interesting 'side-result' of these experiments has been the use of the determined efficiency curve of the cylinder black-body to correct the hitherto unexplained deviation of Ives' Standard of Visible Radiation [2] from other independent determinations. A description of the apparatus together with more detailed experimental results will be published elsewhere. A full summary of all previous work on the optical properties of metals with special reference to the problem of emissivity has also been made, and it is hoped that this will be published at a later date.

The above work forms part of a programme of research being undertaken for the Foundry Steel Temperature Sub-Committee of the Steel Castings Research Committee of the British Iron and Steel Research Association and the Iron and Steel Institute, and with their financial support.

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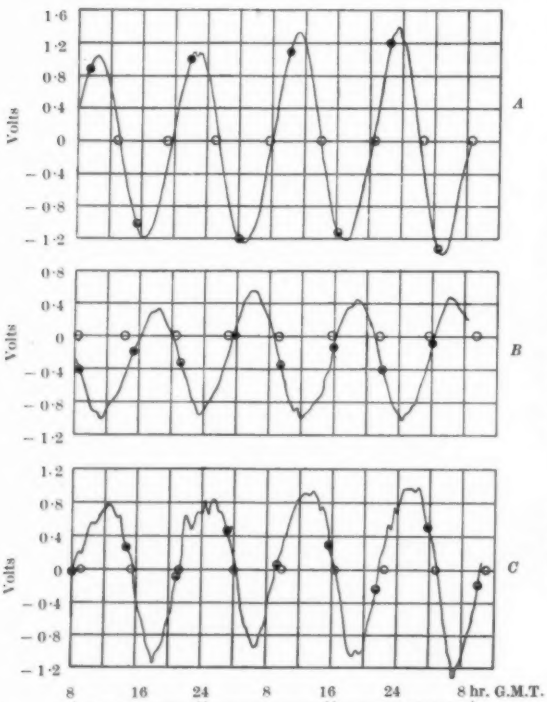
Earth Currents in Short Submarine Cables

PRIOR to 1940, faults on submarine telephone cables between Great Britain and the Continent were normally located by direct-current methods employing a good wire in another cable. Confirmatory tests were usually made by the impedance-frequency method[1]. When the time came in 1944 to restore telephone communications, no good wires were available. The impedance-frequency method was employed for fault localization with great success, and at the same time confirmatory tests were made by direct-current methods which do not require a good wire, such as the tests due to Mance, Kennelly, Black[2], etc. As all of these D.C. tests are affected by earth currents, the nature of this current phenomenon was investigated. The work in general was limited to periods when co-operation was being given to Post Office cable ships in restoring communications, and so the results obtained only cover a limited period; but they are sufficiently consistent to be of interest.

The first series of tests was made on an Anglo-Dutch cable which was earthing some 75 nautical miles from the English end. In order that the measurements should not be affected by electrolytic action, or change of resistance at the fault, if current flowed, it was decided that tests should be limited to measuring the voltage between the cable and earth at the testing end. The apparatus used was a reflecting galvanometer in series with a 1 megohm resistance. The galvanometer deflexion obtained was observed and recorded at regular intervals. The apparatus was then calibrated by applying known voltages and again noting the galvanometer deflexion. As the apparatus used constituted a high-resistance voltmeter, the results obtained are practically independent of the resistance of the cable conductor. Under such conditions the type of cable would not seriously affect the results obtained. At a later stage, measurements were made on cable wires earthed at the far shore. In general, the cables concerned have one centre conductor insulated with 'Paragutta' or 'Telcothene' and an outer concentric conductor of copper tapes which is in electrical contact with the sea water.

The first series of measurements showed that the voltage varied in a semi-diurnal manner, changing from positive to negative values, and these, when plotted, were approximately symmetrical about the line of zero voltage. It was noticed that the periodicity of the variations was the same as that of the tides, and that zero voltage occurred about the time of high and low tide. Results were then obtained on the Dartmouth–Guernsey route. These again had the same periodicity as the tides, but in this case, high and low water occurred about the same time as the peak voltages. This seemed to rule out the possibility of the voltage being due to the rise and fall of the tide; but tests at various times over a period of six months continued to confirm a constant relationship with the tides. Published information[3] on the nature of tidal streams in the English Channel and the North Sea suggested a similarity in the times of change with those of the measured voltages. It appears that tidal stream phenomena have not been determined to any great degree of accuracy, but from information available[4] it seems that the time of tidal stream slack water can be predicted to the nearest half-hour, and investigations showed that in all cases this time approximated to that of zero voltage. The relationships between measured voltage, the rise and fall of the tides, and tidal stream slack water, for three cables measured over the same period are shown in the accompanying graph. The cables concerned run approximately at right angles to the direction of the tidal streams in their area, and slack water occurs at about the same time at all points along any particular cable.



A. Dartmouth–Guernsey 1938 cable. 65·95 naut. miles
B. Aldeburgh–Domburg 1937 cable. 81·6 " "
C. Cuckmere–Dieppe 1944 cable. 69·15 " "
Half-filled circles, mean time of high water
Filled " " " " low " "
Open " " " " tidal stream slack water

Consideration of the direction of flow of the tidal stream shows that the voltage is positive when the stream is passing from left to right relative to the testing end. Unfortunately, the prediction of tidal stream conditions does not appear to be sufficiently accurate to show whether peak voltages occur at the same time as the maximum tidal flow, but it seems to be a reasonable assumption that this is so. Measurements made at both ends of the cable at successive instants show that the voltages are opposite in sign and approximately equal in magnitude. This, of course, is in agreement with the above statement as to voltage sign and direction of flow.

The above considerations are in agreement with the supposition that the centre conductor of the cable acts as a lead between the point at which it is earthed and the voltmeter, and that in effect measurements are made of the potential difference existing between two points of the earth. If the sea water is considered to be an electrical conductor which in moving is cutting the vertical component of the earth's magnetic field, then the sign of the voltage produced as determined by Fleming's right-hand rule would be in agreement with that measured. It has been possible to detect a change in the voltage swing from day to day, and although insufficient tests have been made to confirm this point fully, the changes are not inconsistent with the time of occurrence of neap and spring tides.

The phenomena discussed above could be more fully explored by employing recording voltmeters, but the results already obtained establish the fact that the voltage between the two ends of a short submarine cable is related to the tidal stream conditions existing in that area.

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(I) Relativistic Dynamics of Spin-Fluids and Spin-Particles*

IT is well known that the usual equations of motion of a material particle considered as a singularity of a gravitational field can be deduced from the differential equations of this field only on the assumption of central symmetry in space of the singularity. Mathisson[1] was the first to investigate the consequences of more general assumptions. In case of the field in the immediate neighbourhood of the particle being a superposition of a field due to an infinitely small rotating sphere and a static central symmetrical field, the equations of motion have proved to be of the third order. In the special relativity theory, Mathisson's equations go over into those previously found by Frenkel[2] for a spinning electron in an electromagnetic field (provided that their terms depending on the electromagnetic field are dropped). We have found a third way of working out the same equations, namely, by establishing first the laws of the dynamics of an incoherent spin-fluid and passing then to the limit. However, the form of our equations is much simpler than Frenkel's and Mathisson's, due to the explicit introduction of the four-vector of linear momentum and energy G^α.

By spin-fluid we mean a fluid each element of which is endowed with a certain amount of angular momentum proportional to the volume just as are the energy and linear momentum of that element. We base the dynamics of such an incoherent spin-fluid on the usual equation $\partial_\beta T^{\alpha\beta} = 0$, but put

$$T^{\alpha\beta} = g^\alpha u^\beta \quad . \quad . \quad . \quad (1)$$

where u^α is the four-velocity and g^α is the four-dimensional vector density of linear momentum and energy. No assumption is made as to the relation between g^α and u^α, and thus the energy tensor $T^{\alpha\beta}$ loses its symmetrical character. Finally, we get for a free spin-particle the following equations, where the dots denote differentiation with respect to the proper time τ of the particle:

$$s^{\alpha\beta}u_\beta = 0; \quad . \quad . \quad . \quad . \quad (2)$$

$$\dot{G}^\alpha = 0; \quad . \quad . \quad . \quad . \quad (3)$$

$$s^{\alpha\beta} = G^\alpha u^\beta - G^\beta u^\alpha; \quad . \quad (4)$$

$$G^\alpha = m_0 u^\alpha + s^{\alpha\beta}\dot{u}_\beta/c^2. \quad . \quad (5)$$

* These two communications contain results obtained by me in 1940–41 in Lwów, partly in collaboration with the late Mr. A. Raabe. We tried to pursue our work in 1942 in Cracow; one day in June 1942 we were discussing further details of the theory during a walk in Cracow; we then parted, and Mr. Raabe was never seen again. On his way home he fell victim of a man-hunt and four months later we received the news of his death in the German concentration camp of Oświęcim.

Here m_0, the rest-mass of the particle, has been introduced as $-G_\alpha u^\alpha/c^2$, and $s^{\alpha\beta}$ is the bivector (antisymmetric tensor of rank two) of internal angular momentum or spin of the particle. Equation 2 may be interpreted as the condition of non-existence of negative mass. By eliminating G^α from the above equations one gets the Frenkel–Mathisson equations of motion.

I have since found a still simpler method of deducing the same equations. The main idea can be outlined as follows. Consider a particle of velocity **v** and linear momentum **G**, but do not assume that **G** must be parallel to **v**. The law of conservation of linear momentum yields $d\mathbf{G}/dt = \mathbf{F}$, where **F** is the force acting on the particle. However, the law of conservation of angular momentum (in its familiar form) does not follow therefrom; the differentiation of the moment of momentum (external angular momentum) with respect to the time gives

$$d(\mathbf{r} \times \mathbf{G})/dt = \mathbf{r} \times \mathbf{F} + \mathbf{v} \times \mathbf{G}. \quad . \quad (6)$$

The last product does not vanish as in classical mechanics; we can, however, restore the validity of the law of conservation of angular momentum in a generalized form by putting

$$d\mathbf{s}/dt = \mathbf{G} \times \mathbf{v}, \quad . \quad . \quad . \quad (7)$$

where **s** is interpreted as the three-dimensional spin-vector. It is sufficient to repeat the whole argument in four-dimensional form to get equations 2–4. Equation 5 is obtained by multiplying (4) by μ_α.

The general solution of the equations 2–5 is as follows (Mathisson knew this solution in non-relativistic approximation only): in the inertial frame of reference in which **G** vanishes (we call it the 'rest-system of the circle', Σ_σ) the particle travels uniformly along a circle of radius

$$r_\sigma = \frac{s_0 v}{m_0(c^2 - v^2)} = \frac{s_\sigma}{M_\sigma c} \cdot \frac{v}{c}, \quad . \quad . \quad (8)$$

with the angular velocity

$$\omega_\sigma = \frac{m_0(c^2 - v^2)}{s_0} = \frac{M_\sigma c^2}{s_\sigma}; \quad . \quad . \quad (9)$$

where s_σ and M_σ are the values of s and M in Σ_σ, M being defined by $M = G^4/c$, v is the velocity of the particle in Σ_σ and s_0 is the magnitude of **s** in the rest-system of the particle. In any other inertial frame of reference the path of the particle is a distorted screw-motion resulting from the relativistic superposition of this circular motion and a translation.

Mathisson had noticed already that, by putting $s_0 = \hbar/2$ and m_0 equal to the mass of an electron, the frequency (9) becomes identical with the frequency of Schrödinger's *Zitterbewegung*, but that is so in non-relativistic approximation only. Still another detail points to the fact that the spin particle as considered by Frenkel, Thomas and Mathisson cannot supply an adequate relativistic model of an electron, namely, the radius (8) of the circle on which such a free particle moves can acquire arbitrary large macroscopical values, a result which is obviously contradicted by experiment.

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(2) Spin-Particles Moving with the Velocity of Light

It is well known that, in Dirac's quantum-mechanical theory of the electron, the only eigen-values of the velocity-component operators are $\pm c$. That is why the laws of motion of a spin-particle moving with the velocity of light attract attention even from the point of view of classical relativity theory. Henceforth such a motion will be referred to as the *second case*, whereas the motion discussed in the preceding communication will be called *first case*. The second case may be considered in a certain sense as a limiting case of the first but, in any event, it is not simply the limit of the first when v tends to c at constant proper mass m_0 and constant proper angular momentum s_0.

To obtain directly the equations of motion of a spin-particle in the second case in four-dimensional tensor form, we must first change the parameter along the world line of the particle from τ, the proper time, to an arbitrary parameter p, leaving the world line of the particle unchanged; and only afterwards, as the second step of the reasoning, we may distort the world line in such a way as to make it everywhere tangent to the corresponding light-cone. The first step of the above reasoning brings into the equations of motion the derivative $\tau' = d\tau/dp$, which vanishes at the second step. We get thus the following equations of motion, where the dashes denote differentiation with respect to p.

$$s^{\alpha\beta}w_\beta = 0; \quad . \quad . \quad . \quad . \quad (1)$$

$$s'_{\alpha\beta} = G_\alpha w_\beta - G_\beta w_\alpha; \quad . \quad . \quad (2)$$

$$G'_\alpha = \frac{e}{c} F_{\alpha\beta}w^\beta; \quad . \quad . \quad . \quad (3)$$

$$mw_\alpha + c^{-2}s_{\alpha\beta}w^\beta = 0; \quad . \quad . \quad (4)$$

$x^\alpha = x'^\alpha$ is a 'four-vector depending on the parametrization', which has replaced the now non-existing four-velocity u^α, m, a 'scalar depending on the parametrization' has been introduced as $-G_\alpha w^\alpha/c^2$ and $F_{\alpha\beta}$ is the bivector of the electromagnetic field. The above equations of motion in an electromagnetic field are much simpler than the corresponding equations in the first case, because three of the four additional terms depending on the field disappeared together with τ', and the only additional term left has the familiar form of the four-dimensional expression of the Lorentz force, w^α replacing the four-velocity u^α.

We may take advantage of the arbitrariness in the choice of the parameter p to put the equations into a new form. Put p equal to $t = x^4/c$ in each allowed co-ordinate system: this amounts to changing the parametrization at every change of the co-ordinates in such a way as to make p always equal to t in the co-ordinate system of interest. w^α is then replaced by $v^\alpha = dx^\alpha/dt = \{\mathbf{v}|c\}$; it ceases to be a 'vector depending on the parametrization' and becomes a new geometrical object, which we may call pseudo-vector. The transformed equations are interesting in so far as they give an invariant four-dimensional description of motion (with the velocity of light), though they are not tensor equations.

Though G_α disappeared from equation 4, the new equations of motion may generally be solved as before. Again, in the co-ordinate system Σ_σ in which the three-dimensional momentum **G** vanishes, the particle travels uniformly along a small circle with the radius

$$r_\sigma = s_\sigma/M_\sigma c; \quad . \quad . \quad . \quad (5)$$

and the angular velocity

$$\omega_\sigma = M_\sigma c^2/s_\sigma; \quad . \quad . \quad . \quad (6)$$

M_σ and s_σ being respectively the values in Σ_σ of the mass $M = G^4/c$ and the magnitude of the three-dimensional spin vector **s**.

Obviously s_σ is an invariant, but the question arises whether it remains constant throughout the motion, or whether at least s_σ and M_σ take the previous values when the particle, after passing through an electromagnetic field, comes into a field-free space again? It can be shown that the last constancy is only approximately fulfilled—when the variations of the field are not too abrupt. The physical significance of this fact will be dealt with later[1]. Only the product $M_\sigma s_\sigma$ remains strictly constant in any electromagnetic field (due to the constancy of the magnitude of the vector $s^{\alpha\beta}G_\beta$ or the trivector $s_{[\alpha\beta}G_{\gamma]}$).

The bivector $s^{\alpha\beta}$ fulfils also, besides the relation (1) already noted, the following relations:

$$s_{\alpha\beta}s^{\alpha\beta} = 0, \; s^{[\alpha\beta}w^{\gamma]} = 0, \; s^{[\alpha\beta}s^{\gamma\delta]} = 0. \quad . \quad (7)$$

In spinless dynamics the rest-mass of a particle may be defined as $-G_\alpha u^\alpha/c^2$, or else by the square root of $-G_\alpha G^\alpha/c^2$. In the *first case*, both definitions may be used (giving somewhat different results). In the *second case* considered here, only the last definition remains, as the first would yield infinity. If we put instead $m = G_\alpha w^\alpha/c^2$, as in (4), we should get a 'scalar depending on the parametrization', which could not have any direct physical meaning. A special parameter, π, may be introduced making m constant along the world line of the particle. In field-free space π is proportional to the 'proper time of the circle', that is, to t in Σ_σ, the factor of proportionality varying from one field-free space to another.

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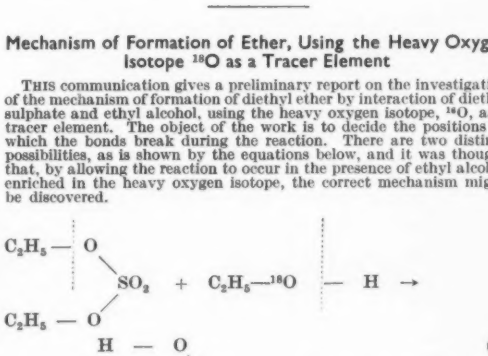
[1] *Nature*, in the press.

Mechanism of Formation of Ether, Using the Heavy Oxygen Isotope ^{18}O as a Tracer Element

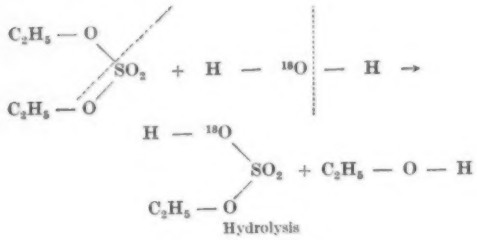
THIS communication gives a preliminary report on the investigation of the mechanism of formation of diethyl ether by interaction of diethyl sulphate and ethyl alcohol, using the heavy oxygen isotope, ^{18}O, as a tracer element. The object of the work is to decide the positions at which the bonds break during the reaction. There are two distinct possibilities, as is shown by the equations below, and it was thought that, by allowing the reaction to occur in the presence of ethyl alcohol enriched in the heavy oxygen isotope, the correct mechanism might be discovered.



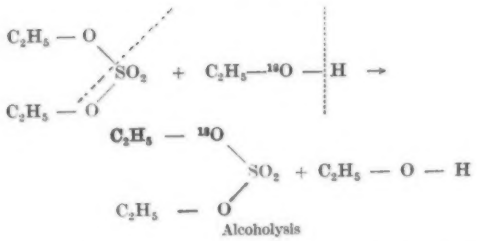
If mechanism (*a*) is correct, and no other interfering reactions occur, the ether formed should contain oxygen with the same isotopic composition as that of the original alcohol. On the other hand, if mechanism (*b*) is correct, the ether formed should contain 'normal' oxygen.

This etherification reaction[1] occurs readily at 57° C. but more readily, even at room temperature, if an equivalent amount of alkali and a small amount of water are present[2]. As a first step, the investigation is being carried out under the latter conditions. The reagents are mixed in the following proportions: 3·0 millimoles of diethyl sulphate, 2·5 millimoles of ethyl alcohol, 2·5 millimoles of sodium hydroxide, 3·0 millimoles of water; and, at 30° C., the reaction is half-complete in 1·1 hr. The diethyl sulphate contains normal oxygen, while each of the other reagents contains an additional 0·5 per cent ^{18}O above the amount normally present.

The interpretation of the results is complicated by the fact that, besides the main reaction leading to the formation of ether, two side reactions are involved, namely, hydrolysis and alcoholysis. The mechanisms of these reactions have not been investigated as yet, but, judging from similar cases[3,4], it is probable that each leads to the formation of 'normal' ethyl alcohol:



(*c*)



(*d*)

'Normal' alcohol produced by these reactions would dilute the heavy oxygen alcohol initially present in the mixture.

It may be noted that, under the above experimental conditions, the etherification reaction and also the hydrolysis reaction lead to the formation of sodium ethyl sulphate but not sodium sulphate. The stoichiometric equations for these reactions are thus:

Etherification. $(C_2H_5)_2SO_4 + C_2H_5OH + NaOH \rightarrow (C_2H_5)NaSO_4 + (C_2H_5)_2O + H_2O$ (*e*)

Hydrolysis. $(C_2H_5)_2SO_4 + NaOH \rightarrow (C_2H_5)NaSO_4 + C_2H_5OH$ (*f*)

The experimental results show clearly that the ether formed during the reaction contains more than the normal proportion of heavy oxygen. The exact proportion depends on the length of the reaction period. After a period of 3·5 hr. the ratio $^{18}O/^{16}O$ of the ether is approximately 85 per cent of the corresponding ratio for the original alcohol, while after a period of 35 hr. this figure drops to 60 per cent.

The results are interpreted as indicating that, during the etherification process, the alkyl-oxygen linkage in the ester is broken while that in the alcohol remains intact (mechanism *a*). This conclusion is given some support by stereochemical evidence from a similar type of reaction[5]. The decrease in the mean heavy oxygen content of the ether, as the reaction period increases, is attributed to the formation of increasing quantities of ether from the normal ethyl alcohol produced by the side reactions (*c*) and (*d*).

It may be pointed out that the exchange of oxygen between the ester and the alcohol by mechanism (*d*), followed by alkaline hydrolysis, mechanism (*c*), could lead to the reproduction of the original heavy alcohol.

An experimental investigation of the acid- and base-catalysed hydrolysis of diethyl sulphate is proceeding, and the interchange of oxygen by alcoholysis reactions is also being investigated.

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Two Forms of 1,3 Butadiene

In a previous publication[1], experimental evidence was adduced for the existence of butadiene molecules in two forms, *s-cis* and *s-trans*. This evidence came from a study of the ionization potentials of butadiene gas, both by spectroscopic and electron impact methods. The existence of the two forms has recently received strong confirmation from the thermodynamic studies of Aston, Szasz, Woolley and Brickwedde[2]. A conflict of opinion has, however, appeared as to which of the isomers is the lower-energy form. Rasmussen, Tunnicliff and Brattain[3] concluded that the *s-cis* form had the lower energy; Aston *et al.* followed Mulliken[4] in believing that the *s-trans* was the lower-energy form. We wish to note here that the ionization potential data indicate that the *s-trans* isomer is the lower-energy form.

We have pointed out the very great potential utility in problems of molecular structure of the many molecular ionization potentials now accurately known as a result of the work of Price's school[5] and have shown[6] that it is possible to define 'bond order' in terms of the summed ionization potentials characterizing the orbitals concerned in a bond. For example, butadiene has two π electrons in an orbital χ_1 and two π electrons in an orbital χ_2. In addition it has six σ electrons in CC bonds. For *s-trans* butadiene, the χ_2 and χ_1 orbitals have ionization potentials of 9·02 V.[6] and 11·1 V.[7] respectively, while the σ electrons have ionization potentials close to 12·0 V.[7]. A measure of the total bond order of all the CC bonds in the molecule is therefore taken as 2(9·0) + 2(11·1) + 6(12·0). We have shown that bond order defined in this way increases smoothly with the total CC bond energy.

Now in *s-cis* butadiene the χ_2 ionization potential is only 8·71 V. Electron impact work[7], which clearly indicated this lower χ_2 ionization potential (relative to *s-trans*), gave no indication of a χ_1 ionization potential differing markedly for *s-cis* and *s-trans*. Further, it is safe to assume that the σ electrons will differ little in ionization potential from *s-trans* to *s-cis*. We may therefore take it that *s-cis* butadiene has a probable lower total CC bond order than *s-trans*, that is, has a lower total CC bond-energy. This means—since the CH bonds are expected to have closely the same energy in both isomers—that *s-cis* butadiene must be the higher-energy form.

Thanks are due to Mr. T. M. Sugden for permission to use unpublished data obtained by him.

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Structure of Hyalite and Opal

In the case of silica and its hydrates the physical and chemical phases of the Liesegang phenomenon manifest themselves in a great variety of rhythmic mineral deposits. Among these formations, hyalite and opal appear to be based upon a common colloidal substrate identical with the prototype, gelatinous silicic acid[1]. There is no such agreement, however, regarding the differences between these two minerals. Thus, the existing theories of opalescence ascribe the display of colours to (1) internal fissures (Haüy[2]), (2) air-pockets or microscopic cavities (Brewster[3]), and (3) incorporated curved opal lamellæ (Behrens[4]). These theories appear to enumerate contrasting possibilities rather than elucidate realities, and certainly bear no relation to the conditions underlying the formation of the minerals. The absence of clear-cut differences (other than optical) between hyalite and opal in their state of hydration, specific gravity, and tensile strength, as well as the combined effect of environmental pressure and syneresis, are opposed to Haüy's and Brewster's theories. Any cavities or fissures which may occur would be merely adventitious and a source of weakness.

The recent synthesis[5] of the clear, opalescent and translucent varieties of gelatinous silicic acid (secondary polymer, *p*H less than 7·0) under conditions specific to the Liesegang phenomenon throws considerable light upon this problem. It co-ordinates the structural and genetic relationship between these three varieties (differing in size, number and orientation of amorphous silica particles) with the naturally occurring hyalite, opal and milky opal.

On this basis further evidence against the theories of Haüy and Brewster may be found in the fact that the appearance and the general properties of opalescent and translucent silicic acid gel[5] are not affected by such previous treatment of the initial reagents as boiling and cooling *in vacuo* of the waterglass and the redistillation of the hydrochloric acid.

Behrens' conception of the structure of opal is not borne out either by the constitution of the three forms of silicic acid genetically related to it, or by the mechanism of their formation; nor does it provide a demarcation between the structure of hyalite, opal and their silicic acid precursors. Behrens' opal, in fact—a composite of two forms of hydrated silica (with the possible admixture of quartz and iron oxide) varying in density and refractive index—should be viewed as an aggregate of silica lamellæ orientated within the network structure of a clear hyalite medium, which itself may be regarded as the pure tertiary polymer of silicic acid.

The evidence of a crystalline structure provided by the X-ray powder photographs of opal[6] (which has a direct bearing upon the problem before us) seems to be inconclusive. We are still uncertain whether the revealed crystalline structure is superimposed upon the amorphous mass as a general pattern of the opal organisation, or is merely a record of local anomalies or deformations. In any event, the assumption of a heat-treatment for opal as a pre-requisite to the occurrence within it of cristobalite forms or quartz is not essential. If of occasional appearance, such silica particles may constitute the

disintegrated but unreacted portion of the country-rock embedded in the silicic acid gel (the latter being the product of the action of magmatic waters upon the rock). On the other hand, the demonstration of a regular crystalline silica constituent in opal would lend additional support to our conception, that is, the practically simultaneous evolution of silica and silicic acid to form opalescent silicic acid, the first stage in the formation of opal.

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Nutritional Value of High-Extraction Wheat Meals

THE world-wide shortage of cereals has made it urgently desirable to attain a clear understanding of the nutritional value of bread prepared from high-extraction meals; hence it was thought that the following information, based on experience gained in South Africa, might be of value. In this country the old custom of making bread from meals of 90–100 per cent extraction, which are ground in small local mills, has lingered on in many of our rural areas. Moreover, since May 1941 all meal used for bread-making has been standardized at an extraction-level of at least 95 per cent—usually somewhat higher.

Naturally nutritionists here have been disturbed by the evidence brought forward during the last few years, indicating that the high phytic acid content of breads made from such meals has a deleterious effect upon the absorption of mineral salts; this leads not only to the poor utilization of constituents such as calcium, but may also adversely affect the availability of this element present in the remainder of the diet. This is all the more disturbing in a country where wheat and maize constitute such a large proportion of the diet, and where other sources of calcium are apt to be consumed in very small quantities.

The National Nutrition Council therefore decided to carry out investigations similar to those on which the above evidence is based. For this purpose we employed healthy human subjects who consumed 1 lb. of our standard war bread daily. We have closely followed the methods and general procedure described by McCance and Widdowson (1942-43)[1], and have measured the intake and excretion of calcium, magnesium, total phosphorus and phytic acid phosphorus for three adult males over somewhat lengthy periods. As suggested by these workers, iron balances were also carried out to check the accuracy of the experimental and analytical technique employed. Full details will be published elsewhere, but our conclusions for calcium may be briefly summarized as follows:

(*a*) Subjects passing abruptly from their usual diet to one of lower calcium content and including a considerable amount of bread made from a high-extraction meal show an immediate negative calcium balance. Gradually, however, the body adjusts to the new conditions, so that, *given time*, it again begins to retain calcium and in due course the losses sustained are made good.

(*b*) The period of adjustment is shorter, and the loss of calcium reduced, if the disparity between the usual calcium intake and that of the experimental diet is small, and if the calcium intake is maintained throughout at a level of about 10 mgm. per kilo body-weight.

(*c*) Immediately on passing from the usual to the standard bread diet, it was observed that the phytic acid ingested was almost completely hydrolysed; on returning to the usual diet, with its lower phytic acid content, the proportion hydrolysed fell to that previously observed.

The foregoing may be illustrated in part by the figures obtained for one of our subjects who was studied continuously for 22 weeks; during the first two weeks, the usual diet was consumed, followed by nine weeks in which 1 lb. of war bread was eaten daily.

Subject's weight 51 kgm.

		CALCIUM		
		Intake (mgm./day)	Absorbed (per cent)	Balance (mgm./day)
Usual diet	Week 1	1160	27	+66
	,, 2	1174	36	+54
War bread diet	Week 1	510	21	−86
	,, 2	512	24	−78
	,, 3	524	29	−69
	,, 4	501	40	−65
	,, 5	509	50	+29
	,, 6	515	38	−22
	,, 7	509	37	−13
	,, 8	561	48	+47
	,, 9	488	47	+35

In this case, the sudden and drastic reduction in calcium intake was followed by a fairly prolonged period of adjustment. With another subject, whose usual calcium intake was considerably lower and did not differ appreciably from that provided by the experimental diet, it was found that adjustment to the phytic acid contributed by the bread has been established by the third week. It must be borne in mind, however, that our subjects had been consuming war bread for several years, and to some extent may have become habituated to an increased ingestion of phytic acid.

The ability of the body to accommodate itself to varying levels of intake has been emphasized by various workers (for example, Steggerda and Mitchell[2]); it therefore appears essential in such work to continue observations for a considerable period before drawing conclusions, as has been advocated by Kraut and Wecker[3]. The point we wish to stress, however, is that the presence of fairly substantial amounts of phytic acid in the diet does not prevent such an adjustment taking place.

Indeed, this would appear to have been already established by the observations of other workers. Thus Nicholls and Nimalasuriya[4] studied a child of seven years old for a period of nine days, who was living in Ceylon on the usual diet consisting mainly of cereals and pulses, both of which are known to be rich in phytic acid: the daily intake of calcium was only about 0·2 gm., yet he managed to absorb no less than 79 per cent of this meagre supply. Similarly, Basu *et al.*[5] found that two out of three adult Indians studied, who were consuming equally small amounts of calcium obtained from rice and wheat, absorbed as much as 50 per cent and were even in positive balance. In these examples, accommodation to these diets had presumably been lifelong. Cruickshank *et al.*[6] reported that the phytate-phosphorus of oatmeal was almost completely digested by four adult subjects when the calcium intake approached requirement. Here, as with our own subjects, it was the actual process of adjustment that was under observation.

Only along such lines can we explain why the poorer Europeans of South Africa and particularly the Bantu population, who live almost exclusively on very high cereal diets, are able to absorb the small amounts of calcium present when accompanied by such large amounts of phytic acid. We are planning to carry out similar balance experiments on healthy Bantu subjects.

While we do not wish to minimize the necessity of providing an adequate amount of calcium in the diet, we do suggest that if, for emergency reasons, such high extraction meals must be employed, the disturbance of calcium metabolism which undoubtedly does occur may be regarded as of a temporary nature.

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Splitting of Adenosine Triphosphate by Myosin

ADENOSINE triphosphate is split by recrystallized myosin into adenosine diphosphate and free phosphate; the adenosine triphosphate used and the diphosphate formed corresponded to Lohmann's formula. One of the 'phosphates' of the adenosine triphosphate is hydrolysed off easily (in 7 min. at 100° in normal hydrochloric acid), while the other is stable (N/P, 1·13; number of acid-equivalents, 3). If one 'phosphate' is split off, one additional acid-equivalent is formed. The NH_2 group can be liberated by desamidase or nitrogen dioxide.

If the reaction is carried out in presence of a protein, 'adenosine triphosphate-isomerase'[1] and a dialysable factor, the diphosphate and monophosphate (adenylic acid) are formed in quantities depending on the duration of the experiment.

The adenosine diphosphate thus formed differs from that described above. One of its 'phosphates' is split off in 60 min. (100°, in normal hydrochloric acid), whereas the other takes three hours (N/P, 1·13). The NH_2-nitrogen is not liberated by desamidase or nitrogen dioxide. If one 'phosphate' is hydrolysed off, no new acid-equivalent is formed. The adenosine monophosphate also differs from 'muscle-adenylic acid'. Though N/P is 2·26, it has no free NH_2, no nitrogen being liberated by desamidase or nitrogen dioxide. It seems probable that during the experiment an adenosine triphosphate is formed which contains no free NH_2.

A detailed report will appear after conclusion of the research in the *Acta Hungarica Physiologica*.

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A Possible Alternative Orientation in Protein Monolayers

LITTLE is yet known regarding the change undergone by a native protein when it is adsorbed at an interface. The evidence at present available, which is largely based upon the preservation of the activity of enzymes in monolayers[1], would seem to indicate that the molecules are subject to a change of form which remains reversible during the initial period of the adsorption.

I have recently been studying protein monolayers at the cyclohexane-water interface by a modification of the Wilhelmy–Dervichian hanging-plate method[2] in conjunction with the spreading procedure for a surface of constant area described by Alexander and Teorell[3]. It was found impossible to obtain reproducible force-area curves for native horse hæmoglobin and casein when these were spread from the mixture of propyl alcohol and sodium acetate solution recommended by

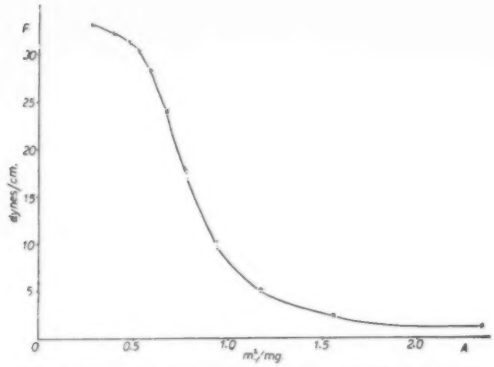

Fig. 1. FORCE-AREA CURVE FOR A MONOLAYER OF HEAT-DENATURED HORSE HÆMOGLOBIN AT THE INTERFACE CYCLOHEXANE/0·066 *M* PHOSPHATE BUFFER (pH 6·81). TEMPERATURE 18° C. FOUR CONSECUTIVE DETERMINATIONS

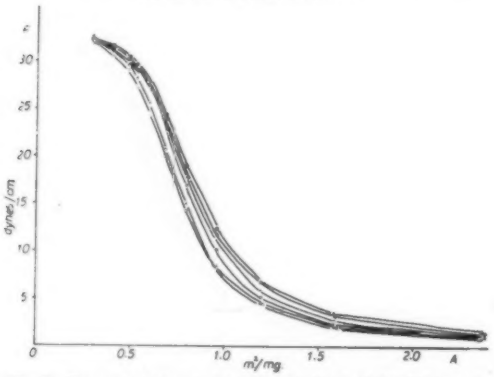

Fig. 2. FORCE-AREA CURVES FOR MONOLAYERS OF NATIVE HORSE HÆMOGLOBIN AT THE INTERFACE CYCLOHEXANE/0·066 *M* PHOSPHATE BUFFER (*p*H 6·81). TEMPERATURE 18° C. SIX CONSECUTIVE DETERMINATIONS

Ställberg and Teorell[4]. When, however, the protein solutions were warmed for five minutes at 80° C. before spreading, a process which brings about heat-denaturation without precipitation, the curves obtained were found to be reproducible within the experimental error (Fig. 1).

A typical set of curves for native hæmoglobin is shown in Fig. 2. It will be seen that at lower compressions the protein exerts film pressures varying on either side of the values for the heat-denatured material. These variations lie far beyond the error of measurement ($\pm 0{\cdot}1$ dyne/cm.) and that of pipetting. There is no evidence, moreover, that spreading is ever incomplete, especially as the apparent area per milligram tends in each case to the same value at higher compressions. We are therefore led to the tentative conclusion that, at an oil-water interface, a molecule of a native protein may assume different orientations, and accordingly make different contributions to the film pressure.

The possibility of such a phenomenon is, indeed, not excluded, if it be supposed that an adsorbed molecule of a native globular protein has a dorsoventral structure. If, as seems reasonable, the unfolded protein network has hydrophilic and hydrophobic groups on either face, with one of these types preponderant on each, there will be possibilities of two orientations at an oil-water interface, assuming the inhibition by cross-linkages of the free rotation possessed by a polypeptide chain, and presumably also in some measure by a heat-denatured protein.

If the more hydrophobic face of the molecule is oriented towards the water, it may be expected that the lateral cohesion of the hydrophobic side-chains will be greater than in the opposite orientation. On the more hydrophilic face the suppression by the oil of the ionization of the polar groups will exert a corresponding effect. The compressibility of the film will therefore be higher when the molecules possess such a reversed orientation, the sense of which may be preserved during compression of the film. This provisional interpretation would appear to be concordant with the experimental findings.

The randomness shown by the force-area curves for the native protein is difficult to explain. Even though the usual precautions have been observed for the maintenance of purity, it may be conceived that minute contaminations determine the orientation of some of the first molecules entering the interface, whereafter a two-dimensional crystallization follows about the nuclei thus formed.

It is highly desirable that film potential measurements be made on these systems. Unfortunately, no method is yet available for the simultaneous determination of film pressures and film potentials at the liquid-liquid interface.

The phenomenon has considerable biological interest in that it suggests the possibility of active and inactive orientations of enzyme molecules in cells, and even the display of several different effects by the same enzyme.

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Mode of Entrance of Sodium into the Aqueous Humor

Kinsey and Grant[1] have shown that the dynamics of accumulation of various electrolytes from the blood into the aqueous humor of the eye can be explained on the assumption that the rate of entrance is proportional to the concentration in the blood but independent of the concentration in the aqueous humor itself. They consider this to prove that electrolytes enter the eye by secretion. However, real ultrafiltration, involving bulk movement of fluid (as contrasted to dialysis), would give the same relationship. Their work therefore only shows that electrolytes do not enter the aqueous humor by diffusion.

In order to decide between the two alternatives, ultrafiltration and secretion, I have studied the influence of arterial blood pressure on the rate of accumulation of the isotope sodium (24) in the aqueous humor. Eight rabbits with the common carotid of one side occluded received sodium (24) intraperitoneally. Aqueous samples were taken from both eyes 40–60 minutes after the injection. At this time, about 50 per cent of the plasma-level was reached in the aqueous humor of both eyes and *there was no significant difference between the two sides.*

Occlusion of the common carotid reduces the mean blood pressure in the ipsilateral ophthalmic arteries by at least 30 mm. mercury. This would cause a large decrease in the rate of exchange of any aqueous constituent entering the eye by ultrafiltration. In conjunction with the findings of Kinsey and Grant, the experiment thus proves that sodium enters by secretion.

A fuller discussion of these experiments, which are part of a wider investigation into the relation between the vascular pressures and the fluid exchange of the eye, will shortly be published.

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Antioxidant Effect of Commercial Lecithin in Fortified Margarine

THE antioxygenic activity of several antioxidants on the preservation of vitamin A and carotene in various vegetable oils and hardened fats has been studied. Some of the results of these experiments which have been tested for their practical application in the preservation of vitamin A in fortified margarine are dealt with below.

The tests, which have been carried out on a semi-commercial scale at the Palestine Margarine Manufacturers Corporation, Ltd., Tel Aviv, Palestine, proved that commercial soya bean lecithin, commonly used as an emulsifier and stabilizer in margarine fat mixture, is quite an efficient antioxidant for vitamin A-fortified margarine, provided that the vitamin is incorporated at a proper stage in a carefully processed margarine.

The process of margarine manufacture[1] consists in the emulsification of a heated mixture of melted hardened fats and oils with or without milk, by the addition of lecithin. The emulsion is then cooled to a butter consistency and kneaded. In our tests only coco-nut oil, cotton-seed oil, ground-nut oil and their hardened fat variations have been used. Commercial soya bean lecithin from the United States at the rate of 0·25 per cent has been added. A fish liver-oil vitamin A concentrate containing 300,000 I.U. per gram has been incorporated at the rate of 30 I.U. per gram of margarine. The presence of vitamin A and its content in the fortified margarine has been determined by the Carr–Price antimony trichloride colour test compared with that of fresh butter and unfortified margarine.

Using batches of 100 kgm. each time, the following procedure in the preparation and fortification of margarine has been used. With the addition of a proper amount of water, the hardened fat and oils mixture was heated to a temperature not exceeding 40° C. and mixed mechanically to an emulsion for about 15 minutes. A calculated quantity of vitamin A concentrate was added to the lecithin, which was previously dissolved in a small volume of the warm fat mixture. This vitamin-lecithin mixture was mechanically agitated for a short time and added to the margarine fat mixture shortly before cessation of the mixing. The mixture was then cooled and kneaded.

By this procedure a fortified margarine was obtained which showed almost the same original vitamin content even after six weeks. It should be noted, however, that the addition of some natural antioxidants proved to be more effective than lecithin alone. The vitamin A content has been controlled at the beginning and after each week during a period of two months.

The presence of vitamin A could not be shown 3–6 days after the preparation of a margarine in which the vitamin and lecithin are added to the bulk fat mixture at the beginning of the mixing, or at a temperature exceeding 40° C. (even if antioxidants such as iso-ascorbic acid, hydroquinone, gum guaiac[2,3,4,5,6] in various concentration are added).

The vitamin A antimony trichloride blue colour faded also, after a few days only, in a fortified margarine prepared with refined lecithin (by precipitation with acetone from a petroleum ether lecithin solution), according to the procedure described above, with or without the addition of the antioxidants mentioned.

The experiments, which began at the Chemical Department, Cancer Research Laboratories, Hebrew University, Jerusalem, are at present being followed up at the Department of Biological and Colloidal Chemistry of the same university, by the kind permission of Prof. A. Fodor, director of the Department.

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Constitution of Hair Melanins

MELANINS can be extracted from hair by dilute caustic soda solution and purified by chromatography on calcium and magnesium carbonates[1,2]. The extraction with 0·05 *N* caustic soda is easier with rabbit hair and sheep wool than with human head hair. Owing to this ease of extraction, which is important in order to obtain pure melanins, but also because we had in mind an analysis of the phenogenesis of melanic pigmentation[3,4], we have studied chiefly melanins from rabbits of known genotypic constitution. Nevertheless, the similarity of the data for mammalian hair melanins generally with those for rabbit hair melanins suggests that our conclusions are applicable to hair melanins in general and to melanins of other tissues.

The purest melanins have been prepared in the following way: rabbit hairs are well cleaned and treated with alcohol and ether, and extracted with 0·05 *N* caustic soda for a week or less at 37° C., in glass-stoppered flasks (in general, 3 gm. of hair to one litre of solvent). The extraction is stopped before the hairs disintegrate into separated fibres; the purest melanins are obtained in the first three or four days of extraction. The solution is afterwards filtered and chromatographed on calcium carbonate (Merck *præc. leve*) if the rabbit is of the black group (black, wild and grey); or on magnesium carbonate (Pattinson's carb. of magnesia, England) when dealing with brown rabbits or rabbits of the yellow group (yellow, orange and sand). Brown and yellow melanins are not efficiently adsorbed on calcium carbonate, whereas magnesium carbonate retains all kinds of pigment. Development of the chromatograph is by *N* caustic soda, the melanin being tenaciously adsorbed in the upper layer, while disintegrated keratin and more or less decomposed pigment pass through the column[1,2]. Instead of elution, which is not possible after adsorption in these conditions, the melanin is isolated by treating the carbonates with dilute hydrochloric acid. The melanin is now dissolved in 0·05 *N* caustic soda and precipitated with dilute hydrochloric acid; solution and precipitation are repeated a further three times. The final product is well washed with distilled water.

The pigments extracted and purified in this way are easily soluble at *p*H 7·5 or more, for example, in a chloride–bicarbonate buffer. They are melanoproteins formed by closely bound proteins and melanoids[1,2], these latter being the coloured part of the melanins. We have not yet succeeded in decomposing the melanoprotein into its protein and melanoid components, but we were able, by hydrolysis of the protein, to obtain the melanoid intact. After hydrolysis for 48 hr., the melanoid is dried and weighed. For the rabbit the results in Table 1 were obtained.

TABLE 1.

Melanin	Black	Brown	Yellow
Melanoid	55·5 ± 1·0%	41·6 ± 2·0%	30·3 ± 1·7%
Protein (by difference)	44·5	58·4	69·7

Nitrogen determinations (by Kjeldahl) agree with the results of the hydrolyses. The following is an example for black melanin: 96·74 mgm. of melanin with 10·4 per cent nitrogen, or a total of 10·061 mgm. nitrogen, was hydrolysed with 20 per cent hydrochloric acid for 48 hr.; after hydrolysis, 56·62 mgm. of melanoid was obtained with 7·0 per cent, or 3·963 mgm. nitrogen; in the hydrolysate we found 6·275 mgm. nitrogen, while the nitrogen calculated for protein of 15·8 per cent nitrogen is 6·339 mgm.

The melanoids which remain after hydrolysis are almost insoluble in 0·05 *N* caustic soda, but they dissolve when treated with *N* caustic soda for some hours. After dissolution they are precipitated with dilute hydrochloric acid and the dissolution and precipitation are repeated twice. The purified melanoids dissolve readily in 0·05 *N* caustic soda and are different for the three kinds of melanins.

The absorption of the original melanins (melanoproteins) compared with that of the purified melanoids gives results which are in relatively good accord with those of the hydrolyses. According to the colorimetry results at 515 mμ, black melanin has about 60 per cent melanoid and 40 per cent protein, whereas yellow melanin is composed of 30 per cent melanoid and 70 per cent protein. The difference between these values and those of the hydrolyses may be due to a little humin which is adsorbed by the melanoid.

Analyses of the amino-acids of the protein part of the melanoproteins have given the results summarized in Table 2, in which column *a* shows the weight of the amino-acid as a percentage of the dry weight of the protein and column *b* gives the amino-acid nitrogen as a percentage of the protein nitrogen determined in the hydrolysate, assuming that the proteins contain 15·8 per cent nitrogen. The analyses of brown melanin are not yet completed.

We have also analysed white rabbit hair keratin and merino wool by the same methods, and found results in accord with those of the literature for wool keratins.

Comparing the amino-acid composition of the proteins of the melanoproteins with that of the keratins, it is obvious that the melanoproteins

TABLE 2.

Rabbit melanin	Black		Yellow	
	(*a*)	(*b*)	(*a*)	(*b*)
Arginine	6·67	13·59	10·81	22·03
Histidine	1·01	1·73	0·97	1·67
Tyrosine	2·44	1·20	5·52	2·70
Tryptophane	2·68	2·33	3·97	3·45
Cystine–Cysteine	1·21	0·89	0·80	0·59
Methionine	3·24	1·93	1·18	0·70

are not melanokeratids as has been claimed by Stary and Richter[5,6]. By their composition and solubility the melanoproteins are similar to non-fibrous proteins and may be perhaps regarded as belonging to the pseudo-globulins. Like the rabbit melanins, human hair and sheep wool melanins (and also probably *Sepia* melanin) are proteins with a protein and a melanoid part. It seems to us that we can safely conclude that the natural hair melanins are dual compounds of protein and melanoid; the melanoids are perhaps similar to 'synthetic melanins' prepared by several authors[7,8] from tyrosine, phenylalanine and tryptophane. The results of Greenstein, Turner and Jenrette[9] can perhaps be better explained by assuming that natural melanins are proteins formed by protein and melanoid, rather than by supposing a 'contamination' of a pseudo-globulin with a 'melanin'.

Another conclusion from this work is the diversity of the melanins from hairs of different colorations: the difference shown between the keratins and the protein parts of the pigments is also important in connexion with an understanding of certain steps of the phenogenesis of melanic pigmentation.

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Action of Thyroxin and the Response of the Thyroid to Treatment with Sulpha Drugs

THIOUREA and derivatives[1–3] and sulphonamide compounds[4,5] inhibit the formation of the catabolic hormone both in normal and in hyperactive thyroids. In a vain attempt to compensate for the lack of hormone, the gland undergoes hyperplasia, and hypothyroidism with hyperplastic goitre can result[1–6]. A few isolated results suggest that the possibility of the drugs also antagonizing the circulating hormone cannot be excluded[7].

Concerning this question, two series of experiments were carried out. The first was designed to investigate whether the protection afforded by thyroxin against procaine[8,9] is influenced by sulpha drugs. Three groups of young female albino mice of 20 gm. body weight, so far as possible litter mates, received respectively:

(1) A sulpha drug for 11 days at the rate of 5 mgm. per mouse per day. Thiouracil, 4-methyl-2-thiouracil—both known to exert anti-thyroid activity[10]—and two other sulpha-pyrimidine compounds, referred to as *B*2 and *B*8, were given, one of which (*B*2) showed bacteriostatic properties. From the seventh day of treatment, a 0·1 per cent solution of sodium thyroxin in a daily dose of 0·1 mgm. per 20 gm. body weight was added. Each dose of thiouracil and methyl-thiouracil was suspended in 0·1 ml. of a solution of gum acacia and injected intraperitoneally: *B*2 and *B*8 were dissolved in 0·1 ml. of propylene glycol and, like the thyroxin, injected subcutaneously.

(2) Thyroxin as group (1), but without any sulpha drug.

(3) No treatment.

Twenty-four hours after the last injections, the colonic temperatures of all the mice were recorded three times at 15-min. intervals. Afterwards a 2 per cent solution of procaine hydrochloride was injected subcutaneously in a dose of 3 mgm. per 20 gm. body-weight. After this injection, recording of body temperature was continued for two hours.

MAXIMAL FALL OF COLONIC TEMPERATURE DURING TWO HOURS AFTER SUBCUTANEOUS INJECTION OF 3 MGM. OF PROCAINE HYDROCHLORIDE PER 20 GM. BODY-WEIGHT

No. of mice	Mean fall of temperature ° C.	Variation ° C.	Error of the mean	Treatment
20	3·8	2·3–6·2	1·00	none
31	1·5	0·0–2·7	0·66	thyroxin
16	2·6	1·4–3·4	0·69	thyroxin + thiouracil
15	2·8	0·8–4·8	1·03	thyroxin + 4-methyl-2-thiouracil
20	2·7	1·0–4·8	0·89	thyroxin + *B*2
15	2·2	1·2–3·7	0·74	thyroxin + *B*8

The protection exerted by thyroxin (see table) appeared to be smaller in the mice prepared with the sulpha drugs than in those which received thyroxin alone. But only the difference in response between the latter and those treated with thiouracil was statistically significant. It may be of interest that thyroxin significantly antagonizes

procaine, a derivative of *p*-aminobenzoic acid which also interferes with the formation of the thyroid hormone[1].

In the second series, male and female mice of the same body-weight and age as those in the first series, and female rats of 55–60 gm. body-weight, were treated with sulpha drugs on eleven consecutive days in the same dosage as in the first series, that is, 5 mgm. per 20 gm. body-weight per day. The rats received *B2*, the mice thiouracil, *B2* or *B8*. The series contained an additional mouse, the only survivor of an unsuccessful attempt to treat mice and rats in the same way with 2-methyl-2-thiouracil, which was found to be exceedingly toxic, whereas 4-methyl-2-thiouracil, in the dosage used, was practically non-toxic. On the twelfth day the treated animals and the untreated controls were killed, the thyroids fixed in Bouin's solution and serial sections stained with hæmatoxylin–eosin or with Heidenhain's azan.

In the mice, with the exception of those treated with *B8*, the drugs caused thyrotrophic stimulation, especially in that individual which received 2-methyl-2-thiouracil. But the effect was smaller than that which is known to be produced by thiouracil in rats[1] and was mainly restricted to the height of the epithelial cells. The response seems to differ with the species of the experimental animal, for in the rats treated with *B2*, the thyrotrophic stimulation was much more pronounced than in the mice. The contrast with the untreated controls was striking when both treated and untreated rats were kept in surroundings at about 27° C. on the last three days of the experiment, showing that the anterior pituitary, the thyrotrophic activity of which is normally inhibited by a warm environment, was stimulated to increased activity under the influence of the sulpha drug.

The results suggest that the inhibiting influence of sulpha drugs on the formation of the thyroid principle is not regularly combined with an antagonistic action on the secreted hormone, but that some of them may exert this effect in certain circumstances.

The thiouracil and 4-methyl-2-thiouracil were prepared by the Organon Laboratories, Ltd., *B2*, *B8* and 2-methyl-2-thiouracil by Dr. S. Pickholz (Blenkinsop and Co., Ltd.); a report on the chemistry of *B2* and *B8* will be published elsewhere.

O. PECZENIK

Department of Physiology,
University of Liverpool. May 6.

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Lipoid Morphology of the Tubercle

THE characteristic lesion of tuberculosis—the tubercle—consists of epithelioid cells and of one or more giant cells, both derived[1] from the mononuclear phagocytes, and tubercle bacilli may be found within the giant cells and in or between the epithelioid cells. The knowledge of the chemistry involved in this formation can be dated back to Unna and to Sata (see ref. 1), who showed that the tubercle bacilli possess a fatty or waxy envelope. The lipoid of cultures of *M. tuberculosis* was found by Anderson and colleagues[2] to include a phosphatide fraction with which Sabin and her co-workers[3] could produce tubercle-like nodules, with epithelioid and giant cells, on injection into experimental animals. Until recently, the actual chemical sequence in the spontaneous or experimental lesion had not been considerably enlarged upon since Miller noted that the organism was not coloured by osmium tetroxide in Flemming's fixative immediately after injection into the rabbit, although after four days the lesions contain bacilli which are blackened by the osmium. As Miller said, ". . . the bacilli undergo degeneration inside the epithelioid and giant cells"; and Gomori[4] has described one mechanism for this showing that the enzyme lipase in the tubercle was found as granules in the epithelioid and giant cells.

Using a method developed for the demonstration of the structural lipoid[5], it was possible to study the lipoid distribution in the tubercle; and this note describes the appearances in a lesion fourteen days old.

2 c.c. of a thick suspension in saline of a culture of bovine type *M. tuberculosis* were injected into the mesenteric vein of a rabbit, anæsthetized with ether. Fourteen days later, the animal was killed by a blow on the back of the head and small portions of liver immediately placed in the cobalt–calcium–formol fixative. After twenty-four hours, the tissue was transferred directly to 3 per cent potassium dichromate at room temperature for twenty-four hours, and then washed in tap water for ten hours. Paraffin imbedding was carried out by an alcohol–toluene routine. Sections were cut at 4–6 μ and stained for the organism with pyridine–fuchsin, which allows an acid fuchsin–aniline blue counter-stain, or coloured with Sudan black for the lipoid.

Lesions, which were known to be tuberculous by the presence of the typical cellular picture and the organisms, contained aggregations of lipoid in the epithelioid and in the giant cells, the greater concentration being in the latter. In both these situations a diffuse staining of the cytoplasm was seen as well as fine and coarse lipoid granules. The coarse cytoplasmic granules correspond to the description by Gomori of the sites of lipase activity in these cells, and it seems probable that the present method shows in the tubercle the substrate for the enzyme lipase. This has not previously been demonstrated.

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Department of Zoology and Comparative Anatomy,
University Museum, Oxford. May 6.

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Effect of Vitamin E in Coronary Heart Disease

WHILE studying the effect of high-dosage vitamin E (α-tocopherol acetate) on purpura[1], the good influence of this factor upon coronary heart disease became apparent. A study of a series of cardiac patients, carried out with the help of Mr. Floyd Skelton and Dr. Wilfrid Shute, has suggested: (*a*) vitamin E in large dosage (200–600 mgm. Ephynal–Hoffmann–La Roche) has no apparent effect upon normal hearts, even after administration for many months on end; (*b*) its effect upon patients having congestive heart disease and the anginal syndrome is marked; it increases exercise tolerance and diminishes or abolishes anginal pain during the period of its administration; its diuretic effect[2] is pronounced.

The effect of vitamin E upon coronary pain may be produced by a direct action on the coronary vessels or by influencing the metabolism of the heart muscle. The first possibility is suggested by an older observation[3] on the effect of vitamin E in dilating the local capillaries in senile vulvitis. Our work on the purpuras raises another possibility, too. Small hæmorrhages into the walls of the coronaries[4] or into the heart muscle itself may produce such pain. It is now clear that such extravasations may be either prevented or reabsorbed by means of vitamin E.

When vitamin E was given in large doses over long periods of time, some patients complained of cardiac irregularities. These were relieved by reducing the medication to low levels.

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April 27.

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Practical Control of Wireworm with 'Gammexane'

IN their letter on the result of field trials with 'Gammexane', Messrs. Thomas and Jameson[1] claim that, as a result of the application of this insecticide, reductions in wireworm population of up to 65 per cent have been noted. This appears to us to be a somewhat misleading statement unless fully supported by the actual data of the sampling on which the claim is based.

Ten fields in Northumberland, which originally had high populations when in grass, showed in the course of routine sampling for the Wireworm Survey, after one arable crop had been taken in each case, reduction in population as high as 67·73 per cent, with an average reduction of 52·48 per cent. No treatment of any sort save that of good cultivation had been given to the fields.

It should be noted that these figures do not necessarily represent the true reduction, as variation in sampling errors must be taken into account, and this most probably applies to the claim made by Thomas and Jameson.

In the course of trials with 'Gammexane' which are being carried out by us in Northumberland, as yet incomplete, we find that so far the 'plant stand' is approximately 25–30 per cent greater in the treated plots.

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[1] *Nature*, **157**, 555 (1946).

IF further work establishes the effectiveness of 'Gammexane' against wireworms[1], the problem in future will be to decide which fields are to be treated with this substance. Unless it is to be applied to every field containing wireworms (this would mean most of the cultivated land in Britain) it will be necessary to be able to predict in advance which fields are likely to be attacked. I am convinced, as a result of experiences as a member of the wireworm advisory team stationed at Cambridge during the War, that such predictions are more difficult to make than is commonly believed. The assumption that wireworm damage increases with rising population must be used with reservation in making such predictions.

In the first place, estimates of populations can only be useful as a guide to probable damage when they are based on accurate methods for sampling fields and recovering wireworms from the samples so obtained[2,3]. Ordinary methods of counting wireworms picked out from soil by hand are still in use, long after they have been proved totally inadequate for estimating populations. Fortunately, considerable progress has been made in developing methods suitable for this work, and further progress is expected.

Secondly, even when the population has been determined accurately, it is not possible to predict with certainty what the crop result will be. Data on this aspect of the problem now being prepared for publication show many discrepancies between the actual crop results and those expected from the estimated populations. Although failures occur more frequently on fields with high populations, they also occur in disquieting numbers on fields with low populations. On the other hand, fields with high populations frequently show little wireworm damage.

An attempt to examine this aspect of the problem was made in 1945 by comparing the wireworm populations in damaged and undamaged plots in single fields. Of thirteen such fields investigated, only seven showed significant differences in population between the damaged and undamaged plots. Moreover, in comparing the different fields it was evident that there was no level of population at which failures consistently occurred. The undamaged plots on some fields maintained a population several times as high as the damaged plots on other fields. These results support the view that infestation is

only one of the factors which determine whether or not a crop will be attacked by wireworms.

In my opinion, even if the effectiveness of 'Gammexane' is confirmed by the field trials now in progress, considerable wireworm damage will still occur so long as infestation remains the only criterion in deciding which fields are to be treated. There is still a need, therefore, to study the ecological factors which promote or inhibit wireworm attack. Only when these factors are fully understood will entomologists be able to make the best use of this promising new weapon against Britain's most serious insect pest.

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London, W.C.1. May 6.

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A Method for Observing the Life-Cycle of *Melophagus ovinus* (Linn.)

DURING the last thirty years investigators have studied the life-cycle of the 'sheep ked' (*M. ovinus*) in a search for a more efficient control of this pest. By its blood-sucking habit, this permanent ectoparasite causes the sheep to bite and scratch and to rub itself against posts and other objects, with the result that the fleece becomes roughened and soiled.

As the 'ked' will not survive more than about five days away from its host, experiments involve the use of living sheep. This has obvious disadvantages in that the sheep must be isolated, and a very careful search is necessary to find a small number of keds on the fleece. The only alternative is to construct a cage to confine a single pair or a known number of keds to a small area of the fleece. Hill[1] succeeded in confining gravid females for a period of 24 hours in a metal ring 3 in. in diameter and 1 in. deep secured to the skin with pitch plaster: for longer periods this cage proved useless as all the keds escaped. Swingler, Sweet and Seddon[2] and Graham and Taylor[4] were confronted with, and failed to overcome, the same difficulty.

In order to make accurate observations, it is essential to confine the keds to an observable area of the fleece, and by the following technique I was successful in overcoming the difficulty experienced by previous investigators. A strip 2 in. wide was shorn within $\frac{1}{2}$–$\frac{1}{2}$ in. of the skin on the flank of the sheep so as to enclose an area of the fleece approximately 4 in. square. On the shorn area a sleeve of 'mutton cloth' (made by knitting cotton yarn with a plain stitch built upon itself) was secured by means of a rosin–beeswax mixture (80 per cent rosin and 20 per cent beeswax). The open end of the cage so formed was closed by means of a zip-fastener. In constructing this type of cage, it is important to ensure that the area of the skin to which the cloth is attached is not disturbed by the limb movements of the sheep, as strains resulting from these movements soon weaken the attachment of the cloth to the skin, thus allowing the keds to escape.

Four of these cages have been constructed and have proved 'ked-proof' for eight weeks. After this time, it has been found necessary to re-attach the cage owing to the gap caused between the skin and the cage by the growth of the wool. Several aspects of the life-cycle have been investigated with the aid of this method.

The age at which the female ked deposits her first pupæ was ascertained by confining a mature male with a newly hatched female. Copulation occurred 16–24 hr. later; the period elapsing between copulation and pupiposition varied between 12 and 14 days (average 13·4 days). The female deposits her successive pupæ at intervals of seven to eight days. These figures support the work of Graham and Taylor[4] in Australia, who state that the female ked does not become mature until six to seven days after hatching from the pupa. Thus, although copulation occurs soon after emergence, fertilization does not take place until the seventh or eighth day. According to Pratt[5], there are certain anatomical structures of the oviduct which allow the sperms to be stored in the genital tract. This is further demonstrated by the fact that, after one mating, the female continues to deposit pupæ in the absence of a male in the cage.

The duration of the pupal stage of the ked was determined by daily inspection of the pupæ deposited in the cages during the maturation experiments. The region around each pupa was marked by staining the wool fibres with various dyes so that individual pupæ could be easily recognized. During January (average temperature, max. 44° F., min. 38° F.) the minimum period of pupal development was 22 days and the maximum 26 days (average 23·8 days). In Australia, Hill[1] found the incubation period of the pupæ to be 21–24 days in winter (43–47° F.), while Graham and Taylor[4] give 20–22 days (maximum 30 days).

During other experiments, the keds were allowed to move freely over the surface of the sheep. The duration of the gestation period and the pupal stage were similar to the data obtained above. Restriction of the movements of the ked to a small area of the fleece does not therefore appear to affect the duration of the various stages of the life-cycle.

This investigation, which will be reported in detail later, forms part of a combined research programme now being carried out in collaboration with the Department of Animal Health of this College.

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Aberystwyth. April 30.

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Production of Penicillin by Germinating Conidia of *Penicillium notatum*

LITTLE information is available to indicate whether the production of penicillin by *Penicillium notatum* is associated with the growth of hyphal tips, with the elongation of cells behind the growing point, with the metabolism of mature cells or with the autolysis of older, senescent regions. Pontecorvo[1] found the maximum rate of secretion in a well-grown colony to be associated with the region about 1 cm. behind the outer margin, but the nature of the cells concerned still awaits investigation.

An investigation of this question has been undertaken using a method based on that of Brown[2,3]. Large numbers of conidia were harvested from stock cultures and germinated in small volumes of the following medium based on that of Moyer and Coghill[4].

Corn steep liquor	.. ..	10 per cent
Lactose	.. ..	2 "
Glucose	.. ..	2 "
$NaNO_3$	.. ..	0·3 "

The technique found most satisfactory was to hold this medium in glass wool in Petri dishes. Dense suspensions of the conidia were then spread on the surface of disks of permeable 'Cellophane' (see Fleming and Smith[5]) resting on the glass wool. At intervals the 'Cellophane' was removed from duplicate dishes, the conidia were fixed, stained and microscopically examined and the medium was drawn off and assayed for penicillin.

In a first experiment the medium was found to inhibit *Bacillus subtilis* twelve hours after inoculation. This was confirmed by a second experiment in which the medium was assayed against this bacterium at periods of 10, 11, 12, 13, 14 and 15 hours after inoculation, inhibition being again obtained from twelve hours onward. In a third experiment the medium was assayed against *Staphylococcus aureus* at periods of 9, 10, 11, 12, 13½ and 15 hours after inoculation and found to produce some inhibition from 11 hours onward.

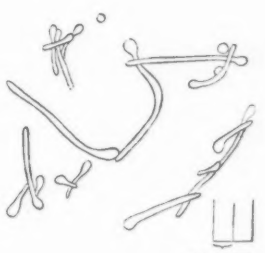

Fig. 1

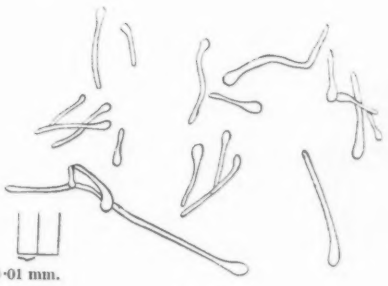

Fig. 2

Figs. 1 and 2 show samples of 11- and 12-hour conidia from the third experiment. These are characteristic of the developmental stage reached at this period in all the experiments, the average length of the germ tubes being about 30 μ. Whether the production of antibiotic substance (presumably penicillin) is associated with the actual tip, with the region immediately behind it or with both cannot be decided from the data so far obtained. The extent to which older hyphal regions may also contribute to penicillin production remains an entirely open question.

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May 7.

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FAZL-I-OMAR RESEARCH INSTITUTE, QADIAN

BOTH in the East and the West, the advancement of knowledge has in the past owed much to the benefactions of religious communities. A remarkable sign of the recognition of the important part which science must play in the India of the future is shown by the decision of the leader of the Ahmadyya community, Hazrat Hkalifatul Masil, to devote a generous part of its funds to the foundation of a research institute. This community is responsible already for a number of educational institutions, including the Talmial Islam College, but its leader now considers the time to be ripe for a further development. The new research institute, the Fazl-i-Omar Research Institute, Qadian, is being opened at a time when India is undergoing a scientific renaissance, and private benefactions have their part to play in making this a success.

The Institute was opened on April 19 by Sir Shanti Bhatnagar, and in an address of welcome, the director, Dr. Abdul Ahmad, gave a short account of its objectives. It is not proposed that the Institute should be devoted entirely to research of an industrial character, since it is clearly recognized that if this is to meet with success it must be based upon fundamental academic research. Situated as it is in an area where agriculture forms the main industry, it is natural that the programme of research on the fundamental side should show a marked biochemical bias, while the industrial research is concerned with the utilization of agricultural products such as vegetable oils. To facilitate the work, the Institute has attached to it a small experimental farm. Dr. Ahmad's address shows that the Institute will have the most up-to-date equipment; but it does not appear to have been fully recognized that of even greater importance is the calibre of the scientific staff which will be necessary if this equipment is to be fruitfully employed.

For more than six years, Indian men of science have been cut off from all outside contacts, and although no one can doubt the rich store of latent ability, this will take time to develop. Throughout the length and breadth of India new research institutes have been opened or are projected, and if these are to be successful they will require leaders of outstanding quality with highly qualified staffs. These are not at present to be found in India, and it is improbable in view of the general dearth of such men in other countries that external recruitment will be possible. It cannot therefore be anticipated that the contribution of this new Institute, whether it be to fundamental science or to the development of industries, will be large for a number of years.

In the brief address delivered by Sir Shanti Bhatnagar before formally opening the Institute, he rightly stressed the point that there is no longer any conflict between science and religion. He emphasized that the approach of science and of the scientific method to industry must be based upon fundamental research, and warned his listeners to beware of the utilitarian type of mind which success in the field of applied science can produce.

We extend a hearty welcome to this new Institute and we do not doubt that in due time it will exert a profound influence on the development of science in northern India.

SOCIAL PROBLEMS OF RE-SETTLEMENT

THE major problem of re-settlement in Britain, and all the implications of the Government schemes for dealing with some four million ex-Service men and women in civilian life, are the subject of two broadsheets issued by Political and Economic Planning. It is doubtful whether either the Forces or the general public appreciate the sheer magnitude of this operation, coupled with inter-industrial transfers, or the many difficulties, particularly psychological, which are likely to arise. The first of these broadsheets, No. 238, "The Disabled Ex-Serviceman", deals with one limited aspect of re-settlement: the question of how the community can best fulfil its obligation to the men and women who have suffered lasting disability as a result of their war service. The main reason for treating this subject separately is that the official schemes appeared to be yielding too high a proportion of failures; but even in this broadsheet it is pointed out that in many respects the distinction between the fit and the disabled is artificial. Both alike need not only the service of efficient re-settlement machinery, but also sympathetic help and advice from officials who like and understand their fellows. Fundamentally, re-settlement is an exercise in the art of treating people as free human beings, not arbitrarily classified units; and the second broadsheet, No. 239, "Re-settlement", is largely confined to a survey of the outstanding needs and problems involved in re-settlement, and a description of the ways in which the Government, reinforced by voluntary effort, proposes to tackle them.

Describing first the Government scheme under the Disabled Persons (Employment) Act, 1941, and the work of the Disabled Persons (Employment) Corporation, the National Advisory Council and the District Advisory Committee, the broadsheet cites several instances of disabled men being offered or directed to unsuitable work by disablement rehabilitation officers. Only through a welfare 'follow-up' were these men eventually placed in satisfactory jobs, and the broadsheet accordingly stresses the need for adequate co-operation between the hospital authorities and the Ministry of Labour, and for welfare follow-up in every case. Much of the criticism in the broadsheet centres on the disablement rehabilitation officer.

The disablement rehabilitation officer is in fact the pivot of the Government's occupational re-settlement scheme, and in view of this evidence that a wrong type of person is frequently being chosen for this work, the broadsheet suggests that, instead of being limited to regular Civil servants from one or a few departments, the field of recruitment should be widened to include, for example, ex-Servicemen who have been doing similar work during the War and who appreciate their fellow-servicemen's point of view, and labour officers from ordnance factories. Another proposal for improving the operation of the schemes is greater attention to the psychological needs of injured men in the early stages of hospital treatment.

The universal follow-up by social welfare workers already mentioned as desirable would in practice mean continuing the work of the field officers and hospital almoners, visiting the disabled men regularly and keeping in touch with the disablement rehabilitation officer until the man is thoroughly settled at

home and in work. The district advisory committee should be formed without delay to focus and give impetus to local work, to provide expert advice for all officials and organisations in any way responsible for the welfare and re-settlement of the disabled men, and to enlist the active co-operation of trade unions and employers. Better co-ordination, both centrally and locally, of the various departments dealing with the needs of the disabled is required, and abolition of the Ministry of Pensions and division of its work between the Ministries of Health and National Insurance is proposed. The formation of a National Rehabilitation and Re-settlement Board, taking over general responsibility for the disabled, with powers specifically delegated to it by the Ministries concerned, should be considered.

The second broadsheet, "Re-settlement", generalizes some of this special argument. Although Government departments realize the need for a truly human relation between their officers and the individual, little progress has been made towards achieving it. Some means of breaking down the sub-humanity of authority is vital, and the implications of re-settlement go far beyond the immense and immediate problems of demobilization. The kind of relations which are established between the ordinary citizen and the Government officer in the process of re-settlement may determine the success or failure of the Government's declared purpose of harmonizing central economic planning with the democratic way of life. Government departments, and particularly the Ministry of Labour, must therefore awake to the contribution which psychology can make in creating a friendly and fruitful relation between themselves and the millions of citizens whom they set out to serve. This is urgent and imperative if seemingly admirable schemes are not to issue in needless bitterness and disillusionment; and it is also desirable that in the Forces the period before release should be treated primarily as a period of preparation for re-settlement.

The careful examination of the whole psychological background is one of the most valuable features of this admirable broadsheet and deserves to be studied thoroughly. The special need of sympathetic treatment of air-crews while they are doing routine duties prior to demobilization; the need of the returning men to be on their own with their families, so that they may have a chance to settle down and find their feet, while the only honest course that Government or local authority can take is to tell them that they must be patient and put up, for the time being, with discomfort, minor irritations and sometimes even squalor—points like this are singled out not merely because they call for sympathetic Government action but also because so frequently the attitude and action of their fellow-citizens can help by demonstrating that a real effort is being made to meet their needs as quickly as possible. Special reference is made to the position of those men of good education who had, or would have, entered industry in peace-time, or learned a practical profession for which a university qualification was not necessary. They will now have little background knowledge and they may have to start completely afresh, learning the elements of their particular industry. They may have five or six years of service to their credit; many of them will have held commissions or positions of responsibility. There may be a good case for concessions to such men comparable to those granted to university students whose training has been interrupted, and this is one of the problems at present under consideration by Sir Frank Newsome Smith's Committee.

Like the previous broadsheet, in considering the Government schemes it is pointed out that good though the background may be, the success of the service itself will depend on the officers who operate it, and the selection and training of staff, particularly those who are in actual contact with the public, should receive extremely careful attention. Much has been learned during the War of the use of applied psychology in personnel selection, and the wider adoption of such methods is strongly advocated. Again, as regards the relatively small number of interviewers at appointment offices, these should be university graduates, and they should be familiar with the variation in academic value of degrees gained at different universities. High-grade officers should have full knowledge regarding all questions bearing on their work which may be under consideration by inter-departmental committees or *ad hoc* bodies; this is not always so at present.

Although the Government schemes go far to meet the requirements of demobilized men and women, there will still be scope and need for voluntary action, in the opinion of *Planning*, and the arrangements for pre-release training and education are not regarded as adequate. Except for really keen students, they are unlikely to provide more than a very superficial smattering of knowledge due to the inadequate time allotted, the liability of arrangements to be upset by troop movements and excessive dependence on the attitude of senior officers to education. Moreover, pre-release training and education can only be of secondary importance to Service authorities, the primary responsibility of which must be to maintain efficient regular forces. The Services can never be the ideal authority to undertake educational and training schemes for civilian life, but education officers should be given every possible encouragement; quiet places should be provided for study; and the present weekly average of six hours allotted to the schemes should be substantially increased.

Much of the value of this pre-release training will be lost unless there is an opportunity for direct continuance of education in civilian life; and a growing demand is likely to arise for informal education through talks and discussions on the lines of the A.B.C.A. programme. The direct provision of informal education must be left to voluntary bodies, which must embark on a programme of improvisation to meet the immediate need. The responsibility for vocational training rests jointly on Government and industry.

Voluntary aid to re-settlement will be mainly directed towards helping in personal and family problems. The four main needs are housing accommodation, more trained social workers, better co-ordination of the work of voluntary societies, and community centres. In view of the acute shortage of trained social workers, introduction of some shorter form of training than the present two years course is suggested and the enrolment of ex-service people should be specially encouraged. The main criticism of the re-settlement work of the voluntary societies, however, arises from lack of co-ordinated effort, and it is suggested that the local council of social service, or its equivalent, should be the canalizing agency, sending the applicant for advice or help to the voluntary agency which, in that locality, is best equipped to supply it. The citizen's advice bureaux

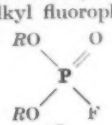
should be an integral part of the co-ordinating arrangement, and men and women in difficulties should have some one place to go to in the first instance; and all concerned, whether officials or not, should know of and recognize this starting-point.

The success of demobilization and re-settlement will not depend only on the efficiency with which the Government schemes are operated, the extent to which the essential human touch is supplied, whether in the appointments office, the vocational training schemes, the employment exchange service, the interrupted apprenticeship scheme or that for further education and training, or on the tact with which the controls necessary to secure the deployment of labour where it is most required are exercised. It will also be affected by the general attitude of those who have not had to leave their homes. It is the great merit of these broadsheets that they can be used to create a wider awareness of the difficulties which are ahead, and that they suggest ways of meeting those difficulties.

ALKYL FLUOROPHOSPHONATES

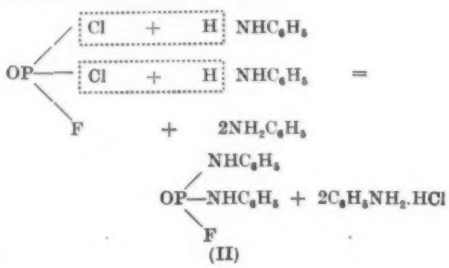
By Dr. H. McCOMBIE and Dr. B. C. SAUNDERS
University Chemical Laboratory, Cambridge

IN *Nature* of March 9, p. 287, we recorded synthetic methods[1] which we had worked out for the preparation of dialkyl fluorophosphonates (I) in high yield. These esters are stable liquids of high toxicity, and have powerful miotic action. The physiological action was found to be enhanced when *R* was a secondary alkyl grouping (for example, *iso*propyl, *sec.* butyl or *cyclo*hexyl). In 1942 we handed several of these compounds to Dr. M. Dixon for test with enzymes. He reported[2] that esters of fluorophosphonic acid inhibit the action of the enzyme cholinesterase, and he showed that di*iso*propyl fluorophosphonate is active in extremely low concentrations (for example, of the order of 10^{-10} *M*). On the other hand, sodium fluoride and ammonium fluorophosphonate require a concentration of 10^{-2} *M* to give 50 per cent inhibition of choline esterase activity. No other enzyme was found, the action of which was inhibited by the alkyl fluorophosphonates.



(I)

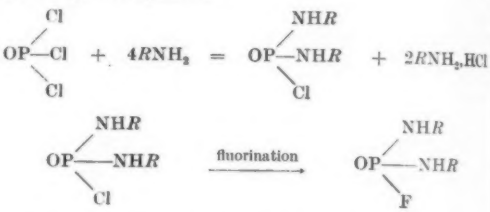
In 1942 we reported[3] the preparation of a new type of phosphorus-fluorine compound, namely, *bis*-anilino phosphoryl fluoride (II). This we prepared by the action of phosphorus oxydichlorofluoride on aniline. The condensation was clear-cut, and only the chlorine atoms were replaced.



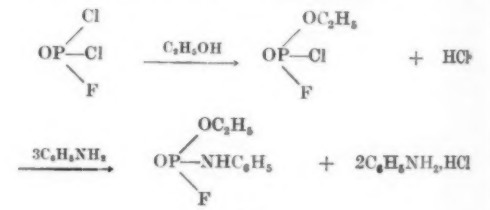
(II)

At a later date, Burg[4] in the United States prepared the corresponding *bis*-(dimethylamino)-phosphoryl fluoride, but used a less direct and less economical process, namely, POF_3 in place of $POCl_2F$. However, he found the compound to be highly toxic, but without miotic action. We then applied our method[5] to the preparation of this compound and found that it worked very satisfactorily. The method was found to be general and was extended to the preparation of amino phosphoryl fluorides from diethylamine, butylamine, methylaniline, benzylamine, *cyclo*hexylamine, and later from morpholine and piperidine. The method was patented[6].

About this time we also worked out an alternative but less satisfactory method of preparing amino-phosphoryl fluorides. The principle of the method is given by the equations:



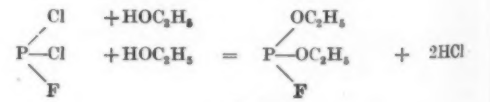
We then conceived the idea of 'combining' the toxicities of a fluorophosphonic ester and of an amino-phosphoryl fluoride in a 'hybrid' molecule, and carried out the following synthesis in 1943[3]:



(III)

This new type of compound (III, ethoxy-anilino phosphoryl fluoride) had quite a high toxicity in spite of the presence of the phenyl group, which is known to reduce toxicity[3]. The L.D. 50 by injection into mice was 12–16 mgm./kgm. At that time we recorded the suggestion[3] that a 'hybrid' compound derived from *iso*propyl alcohol and dimethylamine should be very toxic.

In an attempt[3] to obtain a substance which might have fluoro-phosphonate-like properties and at the same time be more volatile, we prepared diethyl fluorophosphinate (IV) by the action of phosphorus dichlorofluoride on ethyl alcohol



(IV)

The compound, unlike the fluorophosphonate, was readily hydrolysed by water, was relatively non-toxic and produced only very feeble miosis.

Summary. *Bis*-amino phosphoryl fluorides were prepared in this laboratory by the action of phosphorus oxydichlorofluoride on the appropriate amine.

A 'hybrid' compound embracing the essential structural features of the amino phosphoryl fluorides and the fluorophosphonates (namely, ethoxy-anilino phosphoryl fluoride) was prepared in 1943.

We are indebted to the Director-General of Scientific Research (Defence) for permission to publish the above.

The names of members of the team working on these problems have been recorded previously[1].

[1] McCombie, H., and Saunders, B. C., *Nature*, **157**, 287 (1946).
[2] Dixon, M., and Mackworth, J. F., Mode of Action of Fluorophosphonate Esters. Report to Ministry of Supply, April 23, 1942.
[3] McCombie, H., and Saunders, B. C., Report No. 6 on Fluorophosphonates to Ministry of Supply, Sept. 30, 1942.
[4] Burz, by communication, Feb. 19, 1943; and March 15, 1943.
[5] McCombie, H., and Saunders, B. C., Report No. 14 on Fluorophosphonates to Ministry of Supply, Sept. 30, 1943.
[6] British Secret Patent. Ministry of Supply, McCombie, H., Saunders, B. C., Chapman, N. B., and Heap, R., April 17, 1944.
[7] McCombie, H., and Saunders, B. C., Report No. 17 on Fluorophosphonates to Ministry of Supply, Dec. 31, 1943.
[8] McCombie, H., and Saunders, B. C., Report No. 15 on Fluorophosphonates to Ministry of Supply, Dec. 9, 1943.
[9] McCombie, H., and Saunders, B. C., Report No. 18 on Fluorophosphonates to Ministry of Supply, July 4, 1944.

ILLUMINATING ENGINEERING SOCIETY

THE Illuminating Engineering Society in the United States has for many years held annual conventions at which papers on various subjects have been read. This year, for the first time, the Society in Great Britain held a general convention; the organisation of the gathering during May 14–16 was, in present circumstances, an enterprising effort. About three hundred members, many of them bringing friends as guests, registered for the event. Among those present from abroad was Dr. N. A. Halbertsma (Holland), president of the International Commission on Illumination.

The Convention opened on the evening of May 14, when a reception was held at the Institution of Electrical Engineers, where meetings on the two following days also took place. An exhibition, chiefly illustrating special researches on applications of light during the War, had been assembled in the adjacent Lighting Service Bureau, and this was kept open for visitors during the period of the Convention, after members had had an opportunity of viewing it on the opening evening.

Another feature on the opening evening was a series of popular talks, illustrated by lantern slides and ingenious experiments. Mr. R. A. Eshelby, who spoke on "Lighting in Captivity", described the ingenious construction of lighting equipment from refuse material in a prisoners-of-war camp in Germany, especially for the lighting of a small theatre, which the prisoners built for themselves. Mr. Crawford Sugg illustrated the part played by lighting in invasion plans (the harbour area at Antwerp had to be entirely relighted), and Mr. H. R. Ruff and Mr. J. S. Preston lectured respectively on "Some Uses of Black Light" and "Conjuring with Colours". Both these items were illustrated by very effective experiments, Mr. Ruff's demonstration of the Indian rope trick (with a fluorescent rope) and Mr. Preston's demonstrations of the disappearance and reappearance of the letters "I.E.S.", accomplished by a combination of applied scientific knowledge and sleight of hand, being particularly admired.

The holding of the annual general meeting of the Society was followed by a presentation to the retiring honorary secretary of the Society (Mr. J. S. Dow), who has been elected president for the coming session. Afterwards Mr. Percy Good gave an address in which he reviewed researches undertaken by the Society for the Ministry of Home Security during the War (which included such diverse tasks as the provision of war-time street lighting—'synthetic starlight'—and the design of light-traps for the catching of eels). Mr. Good also made some reference to future applications of light as an amenity, for example, for the floodlighting of historic buildings and for the illumination of parks and gardens. Prof. H. Hartridge addressed members on "Light and Vision", illustrating, by the aid of sketches, some of the most recent researches into the structure of the eye and particularly the perception of colour.

At the afternoon session on May 15, over which Sir Charles Darwin, director of the National Physical Laboratory, presided, an address on "New Lamps for Old" was given by Mr. L. J. Davies, who showed many pleasing experiments with fluorescent lamps, illustrating particularly the use of coatings to achieve fluorescence in various colours.

On the following day there were four addresses. During the morning session, over which the Earl of Mount Edgcumbe presided, addresses on "Laboratory Technique during the War" (Mr. W. R. Stevens and Mr. J. M. Waldram) and "The Lighting of Airports" (Dr. S. English) were delivered. The former gave an account of the solution of many problems in visibility in miniature in the laboratory; and the latter illustrated many devices and methods which are expected to play a part in the lighting of airports in the future.

The final session on the afternoon of May 16, over which Mr. Alister MacDonald presided, was given up to two addresses, by Mr. W. Allen on "Daylight and Town Planning", and by Mr. W. J. Jones on "Fluorescent Lighting Practice". The latter was illustrated by many demonstrations and lantern slides of recent installations, for example, that in the underground tube station at Piccadilly.

It is understood that the complete series of addresses will be reprinted eventually in pamphlet form.

It is impossible to do more than summarize very briefly the contents of the Exhibition, which were listed under thirty-nine headings, many containing a number of individual items. A number dealt with applications of light during the War, such as the 'daylight trainer' utilizing sodium light and specially designed goggles to simulate night-flying conditions during the daytime, flashlights to enable photographs to be taken from aircraft at low altitudes, the 'Arditron' photographic flash equipment, a viewing range for naval camouflage (enabling models of vessels to be observed under apparently varied weather conditions) and demonstrations of means of studying recovery from dark adaptation after gun flashes. Methods of lighting instrument dials, special photometric equipment (including low-brightness photometers and 'dirtmeters') and other special instruments were also shown. A demonstration of great interest was the operational recording camera enabling the progress of enemy bombers, and the passage of flying bombs across the English channel, to be traced and viewed. There were also exhibits illustrating the I.E.S. Code, which served as a guide to the lighting of factories engaged on national work

during the War, and problems involved in industrial lighting and in the lighting of homes of the future.

A dinner took place at the Connaught Rooms on the evening of May 15, when the principal guest was the Right Hon. George Tomlinson, Minister of Works. On the evening of May 16, members were the guests of the General Electric Co. Ltd. at Magnet House where an amusing burlesque pageant "Lighting through the Ages" and an 'opera' containing many topical allusions were staged. A visit to the National Physical Laboratory on the morning of May 17 completed the programme.

MATHEMATICAL FILMS

A VERY successful meeting arranged by the London Branch of the Mathematical Association and the Pure Science Committee of the Scientific Film Association took place on May 18 at the Polytechnic, Regent Street, London.

The first film shown presented the Euclidean proof of Pythagoras' Theorem; there seemed to be little advantage in using the film to replace the coloured chalks and the suitable dissections of the figure which would be used by any experienced teacher. The next, on the differential equation $x'' + x = 0$, was not immediately convincing; but a second or third viewing, and a study of the accompanying notes, might make it useful as a 'working model', which was the plan of the producers. On the other hand, the film on the generation of involute gears was quite impressive on first seeing, as was another on the generation of Lissajous figures by the combination of simple harmonic motions. The last of this series illustrated hypocyclic motion and contained several interesting sequences. These films were produced by Mr. R. A. Fairthorne and Mr. B. G. D. Salt, who are the most active of the British producers.

"Rate of Change", produced by Mr. A. D. Segaller, was next shown; in it the differential coefficient was introduced as the slope of the distance-time curve for a motor-car. There followed a French film, "Mouvements vibratoires", which again showed the combination of two simple harmonic motions, and then moving and standing waves on a string. The final item was an American Kodak production on frequency distributions, in which the passage from the histogram to the frequency curve was illustrated in the case of the distribution of the diameters of grains in photographic emulsion.

There is no doubt that the art of the cinematographer has some part to play in the teaching of mathematics; but where that part is and how it should be played is not yet very clear. Several bodies are examining these questions at the moment and this revival of older films will be valuable in the clarification of some of the problems involved. It should now be possible, with more time and perhaps more funds available, to arrange planned studies of the use of films in mathematical teaching. In particular, attention could be paid to the various computing mechanisms which have been developed for fire-control and navigational purposes during the last few years, and for some of which instructional films have been produced for Service use.

Whether the film should be used merely as a quickener of interest in the class-room, or whether it should make a more positive contribution in presenting realizations of the abstract topics under discussion, or whether it should be used in the actual exposition of the topics for which this medium is particularly suited—these are some of the questions which will have to be considered first. Then, a suitable topic for motion-picture presentation having been chosen, it is most important to decide *beforehand* the type of audience to which the film will be shown. Perhaps it will be found that the greatest value to be obtained is in the actual making of films, and this is an activity which can be recommended to clubs and societies at schools, colleges and universities. There are many topics at hand and, in particular, attention might be paid to three-dimensional problems, as has already been done in the United States.

GREENLAND PROBLEMS

IN spite of difficulties in recent years, a number of important monographs on different aspects of Greenland have been published in the series *Meddelelser om Grönland* (København: C. A. Reitzel). "Survey of North Greenland", by L. Koch (*Medd.*, **130**, No. 1, 1940), is a historical and cartographical review in English of all work in northern Greenland, and particularly Peary Land, since the days of the Franklin search.

One of its most important sections is the discussion of the existence of the so-called Peary Channel, which in 1892 Peary claimed to have discovered as separating the ice-clad Greenland to the south from the ice-free Peary Land. This stretched between Nordenskjöld's Fjord on the west and Independence Fjord on the east. Peary did not carry out subsequent plans for its exploration but turned his attention farther north. M. Erichsen failed to find it, and K. Rasmussen in 1919 doubted its existence; but L. Koch in 1921 found that Brönland's Fjord, on the north of Independence Fjord, was continued far to the west in a depression, Wandelsdal, in which lies a lake, Midsommer Sjö, and in 1938 from the air he traced the continuance of this depression not to Nordenskjöld's Fjord but to the longer J. P. Koch's Fjord. Thus, he maintains that the Peary Channel is re-established, not far from the position in which Peary placed it, even if it is not an arm of the sea.

Long before the ice-free Peary Land was discovered, arctic authorities postulated such an ice-free route as affording the only likely way by which the Eskimo could have reached the east coast of Greenland. The conditions of livelihood on the east coast and the scope for population are discussed in a long memoir in English by E. Mikkelsen in conjunction with P. P. Sveistrup entitled "The East Greenlanders" (*Medd.*, **134**, No. 2; 1944). It is now believed that the Eskimo reached the east coast via both the north and the south, but the experiments in neither case seem to have been very successful. Eskimo coming by the northern route had a land-hunting economy, reindeer and musk-ox; those by the southern route a sea-hunting economy, seal and whale. There was more than one migration, at least from the south, between the beginning, about the fourteenth century, and the seventeenth century. From the extreme south to Danmark's Havn, the ruins of at least 1,228 houses have been found, some for five or six inhabitants, others for as many as thirty or forty; but when Europeans came in contact with East Greenland in the nineteenth century, the population was in steady decline. In 1829 it was estimated at 581, in 1892 at

475. Then in 1895, Denmark founded at Angmagssalik the first trading station on the east coast in the only district with any considerable population; and in 1925 some of the population was transferred north to a new station on Scoresby Sound. The problem facing Denmark is that of maintaining an east coast population in view of the dwindling in the past, which seems to have been caused by both decrease of resources, especially whale and seal, and the attraction of the Danish settlements on the south-west where life offered more amenities and so lured the Eskimo from the east. Migration can be checked by the foundation of new Danish stations on the east, but on the other hand a station segregates population, which thus exhausts local hunting and depends more and more on imported food and unsuitable imported clothes. If the population is to survive, it must be induced to spread over wide hunting grounds, and those grounds do exist.

A third important monograph (*Medd.*, **129**, No. 1, 1944) is that by L. Bobé on Hans Egede, the Danish missionary who early in the eighteenth century founded Danish settlements in Greenland and threw light on the fate of the long-lost Norse colonization of Greenland. Old maps and pictures are reproduced.

THE LOOKATOR

ELECTRICAL echoes are known to occur within telephone lines wherever there are impedance irregularities, and it was upon this principle that the 'lookator' was developed for locating faults on telephone lines (J. T. Schott, *Bell Lab. Rec.*, **23**, No. 10, October 1945).

The principle employed is to measure the time for an electrical pulse to travel from some reference point of measurement out to a fault and return in the form of a reflected pulse to the reference point. Knowing the speed of transmission of the pulse over the particular circuit, the distance to the fault can be obtained. In practice, the time is determined automatically by the equipment.

The 'lookator' consists of an oscillator, an initial-zero adjusting circuit, a pulse generator, a hybrid coil with its associated adjustable balancing network, a receiving circuit, a measuring circuit, a sweep circuit, and a cathode-ray tube with associated control circuits. Keys are provided for testing and talking on either a two- or four-wire basis. The bridge-stabilized 220-cycle oscillator feeds through the zero-adjusting circuit into the pulse generator, where the oscillator frequency controls the rate at which the pulses are generated. The pulses, having individually the approximate shape of a positive lobe of a 3-kc. sine wave, are delivered to the line through a hybrid coil and a set of keys. The balancing network forms part of the hybrid-coil circuit, and may be adjusted to balance any line impedances likely to be encountered.

The outgoing pulses travel along the line to the impedance irregularity caused by a fault. They are here reflected, and return along the line to the 'lookator', where they enter the receiving amplifier, and appear as a vertical deflexion on the screen of the cathode-ray tube. A second output of the oscillator feeds through the measuring circuit into the sweep circuit, where it controls the frequency of the horizontal sweep. The zero-adjusting circuit and the measuring circuit permit the phase of the voltage supplied to the pulse generator and sweep circuits of the 'lookator' to be controlled individually. Consequently, the time at which functions in the sending and sweep circuits take place can be adjusted as desired with respect to each other. Since the oscillator frequency is fixed, a measure of the phase difference between the A.C. voltages controlling the electrical events in the two circuits will be a measure of the time required for the pulse to travel from the 'lookator' to the fault and back again. It is then possible to prepare a curve showing the relationship between time in arbitrary scale divisions and the distance out to known points on a particular facility.

In addition to its primary purpose as a fault locator, the 'lookator' can be used for many other purposes such as general laboratory testing, for adjusting networks properly to balance lines, for adjusting line terminations, for observing short-time changes in the height of the ionosphere, and, in its present form, for indicating the presence of large cross-talk coupling and showing approximately where the irregularity responsible for the cross-talk is located.

FORTHCOMING EVENTS

Tuesday, June 11

ZOOLOGICAL SOCIETY OF LONDON (at Regent's Park, London, N.W.8), at 5 p.m.—Scientific Papers.

Friday, June 14

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5.15 p.m.—Capt. W. W. Davis: "Torpedoes, their Use and Development during the War".

Tuesday, June 11—Friday, June 14

MANCHESTER UNIVERSITY (in the Physics Department), at 4.30 p.m. each day.—Dr. Max Delbrück (of Vanderbilt University, Tennessee): "Problems of Modern Biology in relation to Atomic Physics".

Saturday, June 15

INSTITUTE OF PHYSICS, ELECTRONICS GROUP (in the Research Laboratories of the General Electric Co., Wembley), at 2.30 p.m.—Discussion on "The Physics of Luminescence". (Opening speakers: Prof. N. F. Mott, F.R.S., Dr. G. F. J. Garlick and Mr. L. C. Jesty).

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

RESEARCH ASSISTANT IN THE VETERINARY LABORATORY of the Agricultural Department—The Registrar, King's College, Newcastle-upon-Tyne (June 15).

MATHEMATICIAN to take charge of the work done on the Bush machine (differential analyser) and Mallock machine in the Mathematical Laboratory—The Director of the Mathematical Laboratory, Free School Lane, Cambridge (June 15).

TEACHER (full-time) OF CHEMISTRY—The Principal and Secretary, Harris Institute, Preston (June 15).

ASSISTANT LECTURER IN ZOOLOGY—The Registrar, University College, Leicester (June 15).

LECTURER (Grade II), and an ASSISTANT LECTURER (Grade III), IN THE DEPARTMENT OF PHYSICS—The Registrar, The University, Liverpool (June 15).

LECTURER IN GLASSHOUSE CROPS AND COMMERCIAL FLOWER-GROWING—The Principal, Midland Agricultural College, Sutton Bonington, Loughborough (June 15).

ASSISTANT CHEMIST at the River Avon Waterworks—The Water Engineer and Manager, Spon House, 21 Allesley Old Road, Coventry (June 17).

ASSISTANT LECTURER IN THE DEPARTMENT OF BOTANY—The Secretary, King's College, Strand, London, W.C.2 (June 17).

METEOROLOGICAL OFFICER CADETS (15), Department of Industry and Commerce, Dublin—The Secretary, Civil Service Commission, 45 Upper O'Connell Street, Dublin (June 18).

PERMANENT ASSISTANT SECRETARY—The Secretaries, Royal Astronomical Society, Burlington House, Piccadilly, London, W.1 (June 29).

LECTURER IN MATHEMATICAL PHYSICS (including Applied Mathematics) in the Department of Natural Philosophy—The Secretary, The University, Aberdeen (June 30).

MICROBIOLOGIST in the Cider Section, and a RESEARCH ASSISTANT IN MYCOLOGY, at the Long Ashton Research Station—The Secretary and Registrar, The University, Bristol 8 (June 30).

ARCHÆOLOGIST (full-time, male or female)—The Keeper, Yorkshire Museum, York (June 30).

RESEARCH READERSHIP IN ANATOMY tenable at the Medical School, King's College, Newcastle-upon-Tyne—The Registrar, University Office, 46 North Bailey, Durham (July 1).

SPECIAL LECTURER IN THE DEPARTMENT OF PHYSICS—The Head of the Department of Physics and Director of the McLennan Laboratory, University of Toronto, Toronto, Canada (July 1).

READERSHIP IN PHYSIOLOGY tenable at St. Bartholomew's Hospital Medical College—The Academic Registrar, University of London, Senate House, London, W.C.1 (July 2).

LECTURER IN MATERIA MEDICA—The Secretary, The University, Aberdeen (July 11).

ASSISTANT LECTURER IN THE DEPARTMENT OF PHILOSOPHY AND PSYCHOLOGY—The Registrar, University College, Hull (July 12).

LECTURER (Grade I) and a LECTURER (Grade II) IN THE DEPARTMENT OF ANATOMY—The School Secretary, St. Mary's Hospital Medical School, Paddington, London, W.2 (July 18).

LECTURER IN MATHEMATICS AND SCIENCE at the British Institute, Cairo—The Director of Education, British Council, 3 Hanover Street, London, W.1.

LECTURER (full-time) IN CHEMICAL ENGINEERING—The Clerk to the Governing Body, Battersea Polytechnic, Battersea, London, S.W.11.

HEAD OF THE DEPARTMENT OF CHEMISTRY—The Principal, Birmingham Central Technical College, Suffolk Street, Birmingham 1.

PRINCIPAL—The Clerk to the Governors, Harper Adams Agricultural College, Newport, Shropshire.

JAMES S. DIXON CHAIR OF MINING in the University of Glasgow and the PROFESSORSHIP OF MINING in the Royal Technical College (joint appointment)—The Secretary of University Court, The University, Glasgow, or the Secretary, Royal Technical College, George Street, Glasgow, C.1.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Department of Scientific and Industrial Research: Atmospheric Pollution Research. Technical Paper No. 1: Atmospheric Pollution in Leicester, a Scientific Survey. Pp. vi + 161. (London: H.M. Stationery Office, 1945.) 3*s.* net. [1312

Staffing the Hospitals: an Urgent National Need. Pp. 20. (London: H.M. Stationery Office, 1945.) 3*d.* net. [1312

The Scientist and Local Government. By F. Le Gros Clark. Pp. 12. (London: Association of Scientific Workers, 1945.) 3*d.* [1312

University of London: University College. Calendar, Session 1945–1946. Pp. 256 + xxxiii. (London: Taylor and Francis, Ltd., 1945.) [1312

British Cast Iron Research Association. Twenty-fourth Annual Report for the Year ending June 30, 1945. Pp. 16. (Birmingham: British Cast Iron Research Association, 1945.) [1312

University Relief, 1943–1945: a Report. Pp. 16. (London: International Student Service, 1945.) [1312

Imperial Agricultural Bureaux. Joint Publication No. 8: Forest Tree Breeding and Genetics. By R. H. Richens. Pp. viii + 79. (Cambridge: Imperial Bureau of Plant Breeding and Genetics, 1945.) 5*s.* [1312

Colonial Development and Welfare. Despatch dated November 12, 1945, from the Secretary of State for the Colonies to Colonial Governments. (Cmd. 6713.) Pp. 12. (London: H.M. Stationery Office, 1945.) 2*d.* net. [1712

Standing Advisory Committee for Co-ordination of Methods of Survey in Liberated Territories: Dietary Panel. Methods for Dietary Survey. Pp. 23. Recommendations with regard to Methods of Investigation of Nutrition. Pp. 67. (London: Bureau of Nutrition Surveys, London Hospital, 1945.) [1712

Northern Industrial Group. Publication No. 3: The First and Second Reports of the Coal Processing Industries Panel. Pp. 24. (Newcastle-upon-Tyne: Northern Industrial Group, 1945.) 6*d.* [1712

A Reader's Guide to the British Library of Political and Economic Science, London School of Economics and Political Science, University of London. Third edition. Pp. ii + 76. (London: London School of Economics, 1945.) 1*s.* [1712

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Other Countries

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